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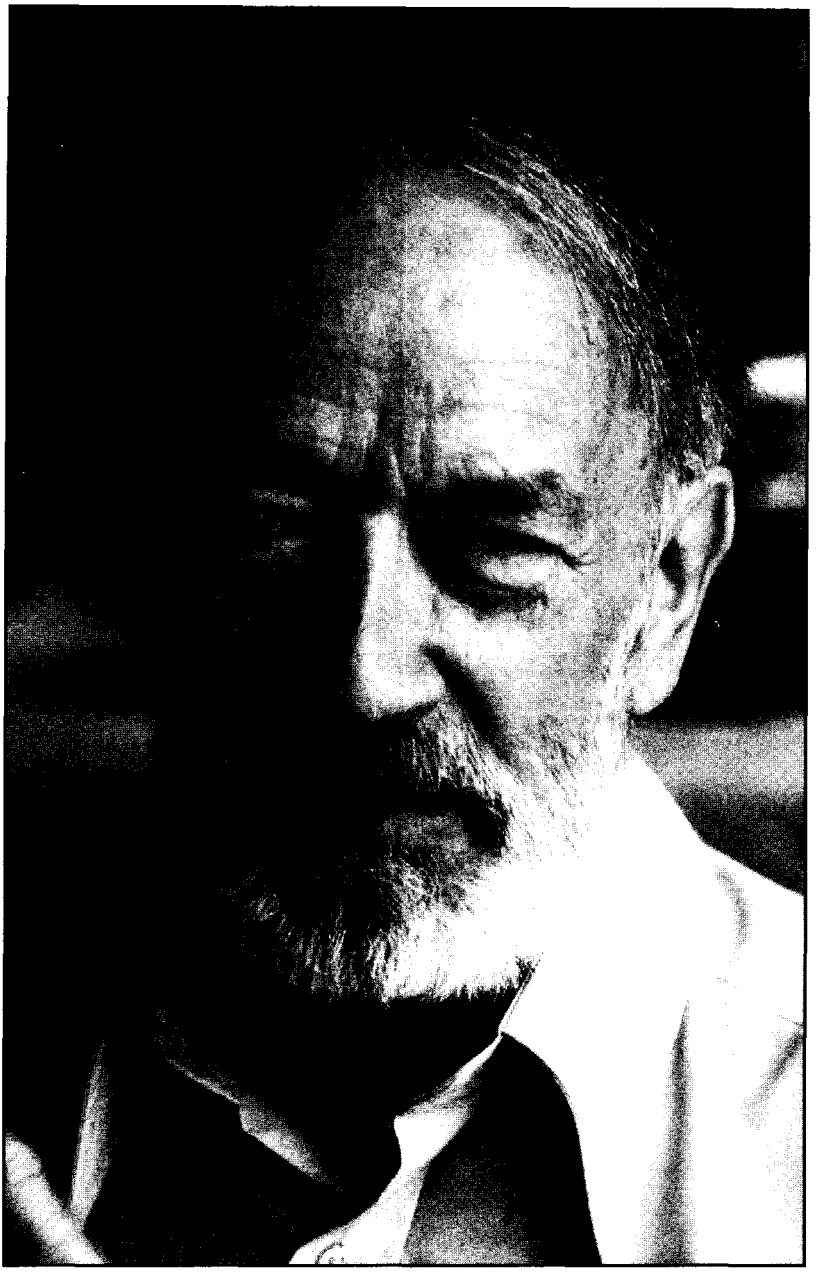

Karl Brunner
1916–1989

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EDITOR'S NOTE

This issue of the *Cato Journal* is dedicated to Karl Brunner, a scholar par excellence and a firm believer in a liberal market order. Karl was a good friend of the Cato Institute and a member of the *Cato Journal's* editorial board.

I had the privilege of meeting Karl in 1983, when I invited him to Cato's first monetary conference. His conference paper, "Has Monetarism Failed?" was an important contribution to the literature. Over the years, I got to know Karl and to admire his intellectual vigor, his integrity, and his courage. He was most at home, it seemed, when he was discussing ideas, especially at his beloved Interlaken seminar in Switzerland.

Brunner, however, was no ivory-tower figure—he sought to put his ideas into action and to influence policy by hard analysis and a close examination of the facts. Thus, he founded (with Allan H. Meltzer) the Shadow Open Market Committee (SOMC) and was an adviser to numerous high-level policymakers here and abroad.

Karl viewed economics as a method of analysis that could be applied to a wide range of human action. Although he is best known for his pioneering work in monetary economics, he turned more and more to fundamental questions of economic and social organization and to the interconnectedness of economics, politics, and morality. He distinguished between the protective state and the redistributive state and derived the implications of each for justice, freedom, and order. For him, as for 18th century liberals and the framers of the U.S. Constitution, social and economic order are best attained by free competition within an institutional framework of limited government, private property, and freedom of contract.

Constitutional economics, property rights theory, and public choice theory affected the way Karl viewed monetary policy. He was highly critical of discretionary monetary policy in a fiat money regime. The lack of a monetary rule, he thought, would create uncertainty in the money-supply process and interfere with rational economic calculation. He did not think the central bank had either the information or the incentives sufficient to fine-tune the economic system through the conduct of monetary policy.

In his view, monetary stability could be achieved over the long run by adherence to rules rather than discretion. And within a constitutional monetary regime, the avoidance of inflation or severe deflation would allow the price system to coordinate economic activity more smoothly than in the current discretionary regime with its stop/go

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monetary policy. Thus, he favored a binding constraint on the central bank's monetary powers, such as limiting the growth of the monetary base.

Karl Brunner represented the quintessential scholar and gentleman. His penetrating mind, youthful spirit, and determination in the face of adversity set him apart as a model of excellence. Many lives have been enriched by his great knowledge and understanding, but even more so by his willingness to listen and explore new ideas, and by his kindness and respect for others. It is with great admiration and fondness of memory that we dedicate this issue of the *Cato Journal* to Karl Brunner.

—J. A. Dorn

The **Karl Brunner Memorial Fund** has been established at the William E. Simon Graduate School of Business at the University of Rochester, Rochester, NY 14627.

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Karl Brunner: In Memoriam

Allan H. Meltzer

Karl Brunner was my friend—a generous, involved, and devoted friend. We shared many interests. Few men in my experience are as fortunate to have had such a long, deep, and lasting friendship. I was his student. Later, as collaborators and friends, we passed much of 36 years in close contact, much of it on the telephone.

There was hardly a week in these years that we did not talk. Almost to the end, Karl was enthusiastic about ideas. Ideas were a major part of his life. "Let's toss it around a bit" was the way he welcomed a new idea.

Our discussions were not primarily social conversations. We used the telephone to go over our work, correct galley pages, and revise manuscripts. We speculated on philosophy, economics, politics, social organizations, and, in recent years, religion—a subject of increased interest to Karl. There were few subjects of interest to either of us that we did not discuss. Karl believed in learning through discussion. He liked both to discuss and to listen. It is not an accident that he founded, cofounded, and organized many conferences in Europe and the United States.

Karl was a gifted teacher who communicated his interest in ideas by presenting them with great enthusiasm. Our association began in a class he taught in logic at U.C.L.A. in the winter of 1953. It continued until the last weekend of his life, when my wife, Marilyn, and I came to say goodbye.

Karl died peacefully in his sleep on Tuesday, May 9, 1989. For me, in retrospect, he died on the February morning of that same year, when he called to say that he could work no more. The pain

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The author is University Professor and John M. Olin Professor of Political Economy at Carnegie-Mellon University and Visiting Scholar at the American Enterprise Institute. This article is reprinted with the author's permission from *A Tribute to Karl Brunner* (Rochester, N.Y.: William E. Simon Graduate School of Business, University of Rochester, 1989), pp. 13–19.

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was too great when he sat at his desk, and he had lost control of his left hand (which he had learned to use when his right hand failed two years earlier). That was a bitter day for Karl and a sad day for me. Karl had had two lodestars in his life—his wife, Rosmarie, and his work. Both were gone, and Karl believed correctly that the end of his life was near. At our last meeting, I asked what to tell his friends in Europe when I went to the Interlaken and Konstanz conferences. He said, "Tell them that my body failed me."

Karl's life was one of accomplishment. He wrote about 200 books and papers, not counting articles for newspapers. He packed the little over 73 years of his life with adventures, achieving renown beyond the reach of most people. Wherever people talk about economics and economic policy, he is known and respected. He worked hard, almost compulsively. He enjoyed his work for its own sake and for the rewards that came from his efforts. And, although Karl read widely, he had some surprising outside interests. When my children were young, he often knew more than I about the adventures of the cartoon character Yogi Bear. He also liked to dance. He had not learned to dance when he was young, however. So he set out to learn in characteristic Brunner fashion: he started by reading a book on dancing. He explained that his approach was to first learn the theory, then do the practice. He also was fond of the opera and theater. During telephone calls on Saturdays, opera would often be playing in the background.

Karl was an adviser to governments and a friend and counselor to central bankers. It is well known that he had a major influence on the policy of the Swiss National Bank. Although he was pleased by the bank's ability to carry out a successful, low-inflation policy, he never claimed credit or spoke of his role. The same was true of his conversations with other government leaders, including Prime Minister Thatcher. On occasion, he persuaded her to reconsider positions proposed by her ministers.

It is important, finally, to speak of Rosmarie. She was his wife, friend, companion, and a major influence for 50 years. She was devoted to him, and he to her. He overcame many obstacles in later years—loss of vision and hearing, and the operation on his spine and partial paralysis. He never overcame Rosmarie's death. Before she died, she had charged him to complete his work. At age 70, when he lost the use of his right hand, he taught himself to write with the left. Despite pain of growing intensity and loss of mobility, he worked to keep his promise. In the two years after her death, he completed two books and many papers, some of which will be published posthumously.

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Karl was born on February 16, 1916. His father was a professor of astronomy at the Swiss Institute of Technology. Karl believed that his intellectual interests and abilities came from his father. His mother was from a peasant family. To her, Karl attributed his not inconsiderable stubbornness and determination. Self-described as an indifferent student in high school, he started to study history at the university, but shifted to economics out of what he described later as a fascination with the subject. He was not motivated principally by a concern for social problems. His concern was man and how he had developed. He sustained the interest throughout his life. One of the papers of his later years has the title, "The Perception of Man and the Conception of Society: Two Approaches to Understanding Society."

Karl's doctoral thesis, completed at the London School of Economics, was "Investigations in the Anglo-Saxon Theory of International Trade." Although he wanted an academic career, there were few opportunities after the war, and no one to help him. His first job was at the Swiss National Bank, followed by a tenure-track position at a research institute at St. Gallen. Although he was discharged in 1948 for his criticism of the quality of the work, the institute awarded him an honorary degree 35 years later.

Karl came to the United States on a Rockefeller Foundation fellowship. After one semester at Harvard University, he moved to the University of Chicago. The years at Chicago were a turning point in Karl's life. Milton Friedman, Aaron Director, and Frank Knight saw economics as more than a set of technical tools about resource allocation in a market economy. For them, economics was concerned with a broader set of issues. It was a way of thinking about society, its institutions, and the relationships between individuals in society. Karl retained this way of thinking throughout his life. At Chicago, he developed his view of economics as an empirical science and his interest in econometrics.

In 1951, Karl began his career at U.C.L.A. and his long friendship with Armen Alchian. He undertook the study of logic and the philosophy of science. It was at U.C.L.A. in the winter of 1953 that I met Karl. Although he later became a leading critic of the Keynesian model, the graduate class in macroeconomics that I took from him was thoroughly Keynesian. Later, he rejected Keynesian economics. During this period, he also began his work on the theory of money supply. Karl's aim was to develop a theory linking the behavior of the central bank, the public, the commercial banks, and other financial institutions to determine the stocks of money and credit. He wanted to join this to an economic theory of aggregate behavior. The main issue for him was the relation of money to income. Karl wanted to

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explain how monetary institutions worked and to understand the effects they had on the relation of money to output.

About this time, we began the early discussions of money as a social institution. A few years later, after much discussion, these ideas were the foundation for our paper, "The Uses of Money." Though deeply involved in work of monetary theory, Karl continued his broader interests. In 1969, he published a paper on the philosophy of science, "Assumptions and the Cognitive Quality of Theories." A few years later, while at Ohio State University, he organized two conferences on econometrics and edited a book of papers on that subject. These are very critical of the foundations of the large-scale econometric models that had begun to appear.

Another strand of his work was motivated by his early interest in human behavior, specifically that of the central bankers and government officials he met in the course of his career. The analytic basis of his model was that of man as resourceful, evaluating, and maximizing—the REMM model, developed with William Meckling at Rochester. This compared the sociological concept of man to REMM, or the economic concept, and applied the idea to the theory of justice and to religion. Karl traced the differences between the two concepts to the differences between the French and Scottish philosophers—one leading to sociology, the other to economics. In the sociological, man was a role player, a passive agent—as Karl would say, an empty slate. In the economic, man was a purposeful, creative being, who responded to opportunities in his environment. In his work on justice, Karl used the REMM vision to compare the idea of justice as a process to the idea of justice as a static state. These ideas underlie man's commitment to rules as opposed to the imposition of authority.

Although I have only touched on Karl's scholarly achievements, it would misrepresent him to ignore the other sides to his life. Many former students, faculty, secretaries, and research assistants remember him as someone who took an interest in their lives. Karl left his mark on people, and he retained friends from all walks of life throughout his lifetime. There is no scale on which to measure his greatness as a person.

What do we learn from Karl Brunner's life? I would have to say, courage. Courage to learn new ideas. Courage to say unpopular things when they are right. Courage in the face of affliction. Karl's courage and personal integrity led him to take up the problems that he thought were important and to pursue them. For him, social science was the way to learn about life and man. More than any other economist I can think of, Karl was a builder. He founded three journals—the *Journal of Money, Credit and Banking,* the *Journal*

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of Monetary Economics, and the *Carnegie-Rochester Conference Series on Public Policy*. He also started the Konstanz Seminar on Monetary Analysis and Policy, the Interlaken Seminar on Analysis and Ideology, renamed the Karl Brunner Symposium, and the Shadow Open Market Committee. All of these activities will continue as the kind of memorials to Karl that he would appreciate and understand.

Karl would tell us not to mourn. One day, he told me that we had spent so much of our lives on the telephone that he undoubtedly would call me from the "other side." Through his memory living in us, that telephone will ring often for me—and for all of us. We won't ever forget this great and good man.

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INTRODUCTION

MONEY, MACROECONOMICS, AND FORECASTING

James A. Dorn

> We suffer neither under total ignorance nor do we enjoy full knowledge. Our life moves in a grey zone of partial knowledge and partial ignorance. Most particularly, the products emerging from our professional work reveal a wide range of diffuse uncertainty about the detailed response structure of the economy. . . . A nonactivist regime emerges under the circumstances . . . as the safest strategy. It does not assure us that economic fluctuations will be avoided. But it will assure us that monetary policymaking does not impose additional uncertainties . . . on the market place.
>
> —Karl Brunner[1]

Some Unsettled Questions

Monetary stability is crucial to economic stability. The smooth coordination of economic activity depends on freely determined market prices and stable money. Erratic variations in the value of money disrupt the market price system by distorting the signaling/information function of relative prices. Individual plans about investment, employment, production, and consumption, in turn, are affected by monetary disequilibrium.

The question arises, therefore, of how monetary equilibrium can best be achieved—by discretionary central banking, by nondiscretionary central banking, or by free banking and currency competition? What are the information and forecasting requirements under alternative monetary regimes? Should we pursue the holy grail of better forecasting or search for better rules?

This issue of the *Cato Journal* discusses those and other questions that bear on the role of money and monetary policy in a free society. In addition, the papers and comments in this issue reassess the

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[1]Brunner (1980, p. 61).

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fundamental changes that have occurred in macroeconomics, examine the record of macroeconomic forecasting, and consider the case for forecast-free monetary institutions.[2]

Monetary Disequilibrium and Monetary Policy

Erratic changes in the quantity of money are the predominant cause of business fluctuations (Warburton [1950] 1966, chap. 1). An excess supply of money, in a world with ragged market-price adjustments, leads to an overheated economy and inflation. Actual shrinkage of the money supply or insufficient money growth, in a world of sticky prices, leads to a business downturn and deflation or disinflation. Monetary surprises affect real economic variables in the short run. However, once individuals adjust to the new quantity of money and nominal values are indexed, changes in the quantity of money will chiefly affect the long-run value of money and have little or no affect on real values or on output and employment. As Kevin Hoover (1992, p. 84) writes, "Only the pathologies of money. . .matter."[3]

The primary task of monetary policy is to maintain a stable growth of money and nominal income consistent with long-run price stability so that markets will not be disrupted by monetary disequilibrium. The history of central banking, however, reveals a failure to maintain monetary equilibrium (see Parkin and Bade 1978). Our current government fiat money regime is fully discretionary: central bankers pursue multiple targets and face no binding rule to constrain money growth to achieve either stable nominal-income growth or a stable price level. As such, the future value of money is uncertain, and "uncertainty emerges as a crucial property of this policy regime" (Brunner 1985, p. 4).

In a discretionary monetary regime, there is greater reliance on macroeconomic forecasting than in a nondiscretionary or rules-based regime. In a private free-banking system, there would be no need for centralized macro-forecasting to guide monetary policy, because there would be no central bank and no monetary policy. Thus, macro-forecasting requirements vary with the type of monetary regime—declining as one moves from government-driven to market-driven monetary regimes.

[2]The papers and comments in this issue of the *Cato Journal* were first presented at the Cato Institute's Ninth Annual Monetary Conference—"Money, Macroeconomics, and Forecasting"—which was held February 21–22, 1991, in Washington, D.C. The conference was supported, in part, by grants from The George Edward Durell Foundation and Ellis L. Phillips, Jr.

[3]For a summary discussion of monetary disequilibrium theory, see Yeager (1986, pp. 369-78).

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The following sections discuss: (1) the state of macroeconomic forecasting, (2) the question of whether monetary stability is more likely to be achieved by improved forecasting or better rules within the framework of central banking and government fiat money, and (3) the implications for monetary stability of moving toward a forecast-free monetary regime without a central monetary authority.

The Dismal State of Macro-Forecasting

The implicit assumption and "fatal conceit" underlying central banking and a government-driven, discretionary fiat money regime is that it is possible to devise macro-forecasting models that are structurally sound and produce forecasts that policymakers can use to fine-tune the economy.[4] According to Martin Feldstein, former chairman of the president's Council of Economic Advisers, "One of the great mistakes of the past 30 years of economic policy has been an excessive belief in the ability to forecast."[5]

The impossibility of knowing the structure of the economy, the difficulty of aggregating economic data and measuring macroeconomic variables, and the fact that every change in policy changes expectations and necessitates a new set of structural parameters and a new macroeconomic model—the so-called Lucas critique[6]—put severe limits on the use of macro-forecasting to guide monetary policy.

The dismal record of macro-forecasting stems from the above-mentioned problems and reveals that macro-forecasting is more an art than a science (see McCloskey 1992). Practitioners of this art have been unable to forecast either short-run swings in real GNP or long-run changes in the price level. As Victor Zarnowitz (1992, p. 158) notes, "The main defects of macro-forecasts from the point of view of policy are the errors of missing cyclical turns and shifts in the average rates of inflation."

Michael Bryan and William Gavin (1991) use the fact that "near-term real GNP forecasts are unlikely to show whether the economy will be strong or weak" to argue that "forecast errors are still too large to justify basing monetary policy on the near-term real GNP outlook alone." They also point to "the fact that inflation has not followed a predictable trend" and suggest that "the FOMC [Federal

[4]On the "fatal conceit" as applied to the government's monopoly of money, see Hayek (1989, pp. 101-4). And on the "fatal conceit" as applied to macroeconomic policy, see Jordan (1992).

[5]Cited in Greenwald (1984, p. 32).

[6]For Robert Lucas's critique of macro-forecasting, see Lucas (1972, 1976).

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Open Market Committee] has often judged that the need to stimulate the real economy in the near term has outweighed the benefits of a stable inflation rate." Indeed, the Federal Reserve has not limited itself to the pursuit of long-run price stability. Instead, the Fed has used its discretion, subject to the prevailing political climate, to select from a broad menu of policy objectives—such as promoting full employment, lowering interest rates, and stabilizing exchange rates—to try to fine-tune the real economy.

Macro-forecasting models of inflation perform about equally well; none are very accurate. In a study for the Federal Reserve Bank of Minneapolis, Lawrence Christiano (1989, p. 3) found that even the new P-Star (P*) model is "not the inflation forecaster's holy grail." The forecast record for the P* model does not differ significantly from the T-bill model and other macro-models.

The poor performance of macro-forecasting calls for a reassessment of the conduct of monetary policy. The Fed basically has two choices: (1) it can continue on its discretionary path and hope to find the holy grail—an accurate macro-forecasting model that provides policymakers with a reliable tool for hitting their selected targets[7]—or (2) it can abandon its attempt to fine-tune the economy—and therefore its activism—and adopt a nondiscretionary rule to guide monetary policy.

The question is: Can macro-forecasting be sufficiently improved to allow the Fed to operate in a discretionary regime or is a better strategy to bind central bankers by a nondiscretionary rule aimed at achieving stable money? Should we aim at better forecasting models within a discretionary regime or work toward improved policy rules?

Better Forecasts or Better Rules?

If the Fed cannot accurately forecast inflation and nominal GNP growth, it cannot hope to achieve its desired target range for both money growth and the federal funds rate (Baird 1981, pp. 250–52). When political pressure pushes the Fed toward lowering interest rates, the Fed will lose control of the money supply—inflation will accelerate and nominal interest rates will rise.

The Fed's macro-forecasting and policymaking may be improved by using "forward-looking market price indicators" such as commodity prices, short-term nominal interest rates, and exchange rates (Angell 1992). However, as Bruce Kovner (1992, p. 195) cautions,

[7]*The Economist* (1989, p. 65) portrayed this challenge by noting, "Economists have long been searching for the holy grail—an accurate thermometer with which to forecast inflation."

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"market expectations can simply be wrong," and "forward prices" may not reflect "the market's 'true' expectations." So while private entrepreneurs may look to supply-side models to forecast the future path of the economy (see Ranson 1992), and policymakers may look to commodity prices and other market price indicators to judge the effectiveness of monetary policy (see Angell 1992), there is no sure-fire way to elevate macro-forecasting to a science.

The Limits of Macro-Forecasting

Better computers and large-scale macro-models have not much improved the art of forecasting. And we should not expect any quantum jump in macro-forecasting accuracy in the future. Rational expectations cannot easily be built into macro-models, so new models become suspect as soon as they are constructed (see *The Economist* 1990, p. 91). The complexity of the economic system, the uncertainty of the exact links among economic variables, and the difficulty of modeling the process by which monetary disturbances affect real and nominal variables narrowly limit the role of macro-forecasting in determining the future state of the economy and in guiding activist monetary policy.

Forecast errors and "an uncertain and probably variable lag between policy action and its impact on the economy . . . make it impossible to be certain that policy based on near-term forecasts will not aggravate the business cycle" (Bryan and Gavin 1991). Moreover, as W. Lee Hoskins (1992, p. 50) argues, "The key to effective policy is not the accuracy of economic forecasts but rather the credibility and predictability of policy actions." That is why Allan Meltzer—who calls macro-forecasting "a weak foundation for policy actions" (Meltzer 1991, p. 31)—and other monetarists have opposed policy activism and recommended a rules-based monetary regime.

The Case for Better Rules

Activist monetary policy has increased uncertainty by making it more difficult to predict the trend in the price level. Moving toward nondiscretionary monetary rules would increase the accuracy of private macro-forecasts and allow the self-regulating market system to function more efficiently.[8] The challenge is to compare alternative monetary rules to discover the rule that would best stabilize the monetary system and allow markets to operate smoothly.

Three commonly proposed rules are (1) a price-level rule, (2) a money-growth rule, and (3) a nominal GNP rule. The first rule would

[8]On the case for a nonactivist monetary regime, see Friedman (1953) and Brunner (1980, 1985).

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target a broad price index to achieve long-run price-level stability; the second rule would target one of the monetary aggregates to stabilize the growth of money and avoid monetary disequilibrium; and the third rule would use the monetary base as an instrumental variable to target nominal GNP so that it grows at a rate consistent with full employment and, perhaps, price stability.[9]

Those who favor a price-level rule argue that since the Fed can only affect nominal variables in the long run, the objective of monetary policy should be to stabilize the price level over time by controlling the monetary base. W. Lee Hoskins (1992, p. 49), for example, favors "a target path for a general price index." He argues, "The Fed cannot eliminate the business cycle; the best it can do is minimize the instability coming from monetary policy by stabilizing the price level." In his opinion, Rep. Stephen Neal's proposal for "zero inflation" (Neal 1992), which would aim at an anticipated inflation rate of zero, would improve the monetary order by abandoning the types of ad hoc "rules" that now prevail, such as targeting the federal funds rate or allowing sharp swings in the monetary base to accommodate political pressures to fine-tune the real economy.

Proponents of a money-growth rule argue that constant growth of a selected monetary aggregate would be a simple rule that would produce long-run price stability. If the Fed had to precommit to a money-growth rule, policymakers could no longer engage in stop/go monetary policy. Monetary uncertainty would be reduced, and the more predictable path of money growth would help the Fed avoid the errors of monetary excess and monetary deficiency that have characterized past Fed behavior. This line of argument assumes, of course, that the long-run velocity of money is stable, so that the changes in money growth translate into predictable changes in nominal GNP and the price level. Milton Friedman, who has long advocated a constant money-growth rule, now believes a zero-growth rule for the monetary base would be the best rule (Friedman 1987).

Those who favor a nominal GNP rule contend that targeting nominal GNP or final demand by an adaptive monetary rule, such as that proposed by Bennett McCallum (1984, 1989), would be superior to either a price-level rule or a constant money-growth rule. Targeting nominal GNP or domestic final sales (DFS) would avoid the problem of adverse supply shocks that increase the variability of output under

[9]A nominal GNP rule, which sets a target path for aggregate demand, "is consistent with any desired price-level path, including a stable price level" (Niskanen 1992, p. 283).

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a price-level rule.[10] A nominal GNP or DFS rule would also avoid the problem of unexpected changes in the demand for money that weaken the link between money and prices and decrease the effectiveness of a constant money-growth rule (see Niskanen 1992).

Although a nominal GNP rule is consistent with any path for the price level, McCallum's adaptive rule would provide for a steady 3 percent growth of nominal income and long-run price stability. Changes in the demand for money would be accommodated by changes in the growth of the monetary base, which also would be adjusted for deviations in the growth of nominal income from its predetermined target path.[11] Thus, although McCallum's nominal income rule would be nondiscretionary, it would be "weakly activist" (McCallum 1989, p. 341) and contingent on the state of the economy (Bradley and Jansen 1989).

The theoretical superiority of the nominal GNP rule could disappear under alternative assumptions, and practical problems could undermine its implementation compared to a *noncontingent* monetary rule, such as Friedman's constant money-growth rule. As Bradley and Jansen (1989, p. 40) point out, changes in the assumptions about the labor market can make a price-level rule theoretically superior to a nominal GNP rule, and "ignorance of the correct equations, parameter values and lag structure that characterize the U.S. economy reduces the appeal of nominal GNP targeting."

It is important to recognize that although adaptive feedback rules like McCallum's are forecast-invariant—in the sense that adjustments are automatic and do not depend on "episodic forecasting"—there is still the need to predict the rule's "operating properties" and to make "a forecast about the economic environment and its interaction with features of the proposed institution" (Yeager 1992, p. 64).[12]

It should be emphasized that all of the above rules, with the exception of Friedman's zero-growth rule for the monetary base, would be engineered by a central bank operating within a government fiat

[10]William Haraf (1986) and Niskanen (1992) advocate adopting an adaptive monetary rule to target domestic final sales.

[11]Allan Meltzer (1989, p. 426) offers a similar adaptive monetary rule that would "keep the anticipated price level constant." He argues that such a rule "would do a substantially better job of stabilizing the economy and avoiding inflation than policies based on short-term forecasts" (Meltzer 1991, p. 32).

[12]McCallum, for example, must make an assumption about "the speed with which deviations of nominal GNP from target are corrected." If the adjustment speed is overstated, the adaptive rule may lead to "dynamic instability" (Bradley and Jansen 1989, p. 38).

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money regime.[13] At a more fundamental level, therefore, the choice is between government-driven monetary institutions, which depend on macro-forecasting, and market-driven monetary regimes, which are forecast-free (see Yeager 1992).

Toward a Forecast-Free Monetary Regime

Nondiscretionary monetary rules, operating within a government fiat money regime, would undoubtedly improve the conduct of monetary policy and reduce the need for macro-forecasting relative to a government fiat money regime with a discretionary central bank. However, further movement to a *nongovernment* monetary regime would eliminate monetary policy, central bank discretion, and centralized macro-forecasting. Private, market-based forecasting would still exist and there would be "macroeconomic entrepreneurs," but there would be no requirement for the type of macro-models now in existence (see Yeager 1992, pp. 70–71).

In contemplating the move toward a forecast-free monetary regime, Leland Yeager (1992) favors a stable price-level regime rather than a stable money-income regime. His proposal for a laissez-faire payments system—formulated with Robert Greenfield (Greenfield and Yeager 1983, Yeager and Greenfield 1989)—would provide for the following: (1) an independent unit of account "defined by a comprehensive bundle of goods and services," so that the value of the unit would not be determined by the demand for and supply of money; and (2) a competitively determined supply of money that would respond to the demand for money at the unchanged price level, because market competition would force private money producers to keep the value of their money stable in terms of the independently defined unit of account (Yeager 1992, pp. 70–71).

Since there would be no high-powered money in the Yeager-Greenfield scheme, there would be no "problem of injection effects" and no "need for central forecasting" (Yeager 1992, p. 70). Monetary equilibrium would prevail and "any forecasting functions that did remain would be healthily decentralized under free banking" (p. 71). Hence, for Yeager, "privatization of money" is "an attractive route toward forecast-free monetary institutions."

George Selgin (1992) also favors a nongovernment monetary regime, but he prefers a "productivity norm" or "stable money-income regime," which he thinks will have even lower private-forecasting requirements than the Yeager-Greenfield stable

[13]Friedman (1987, p. 377) would allow free banking and parallel private currencies to compete with the fixed supply of government fiat money.

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price-level regime. In addition, his final demand rule would avoid the problem of adverse supply shocks associated with a price-level rule.

In Selgin's free-banking system, there is no need for private bankers to forecast movements in future commodity prices, as in the Yeager-Greenfield scheme, because no commodity price index is being held constant. Rather, "stability of income" would occur "automatically as an unintended consequence of . . . free bankers' profit-maximizing behavior" (Selgin 1992, p. 80).[14]

The tendency toward monetary equilibrium under Selgin's free-banking scheme is attractive, as is the independently defined unit of account and monetary equilibrium under the Yeager-Greenfield proposal for a laissez-faire payments system. But neither of these nondiscretionary, essentially forecast-free monetary regimes has seen the light of day; they are theoretical constructs yet to be tested in the marketplace. Nevertheless, historical episodes of free banking in Scotland, the United States (especially the Suffolk system in New England), and Canada indicate that a free-banking system is not inherently unstable.[15]

In contrast, the experience with central banking indicates that monetary disequilibrium has been the rule more than the exception. And there has been little movement in the direction of nondiscretionary central banking. Any connection to gold has long been abandoned, so that today's government money is a pure fiat currency.

Making the Federal Reserve subject to a monetary rule, such as McCallum's adaptive monetary-base rule or a constant money-growth rule, would help discipline the money-supply process, but there would still be no independently defined unit of account. The Fed would continue to exercise some discretion, because, as Yeager (1992, pp. 69–70) writes, "the unit is nothing more definite than a unit of government fiat money managed, one hopes, in some satisfactory way."

Yeager (1992, pp. 70–71) criticizes Selgin's stable money-income regime by asking, "How would you define the unit of account?" For Yeager, "Persons who reject a unit of stable purchasing power and

[14]Other requirements for a free-banking system to "automatically tend to stabilize nominal income" include the following: (1) high-powered money, so-called outside money, must be frozen or grow at a slow rate; and (2) bank money—notes and deposit liabilities, so-called inside money—must be used as the sole payments medium, except for bank clearings (Selgin 1992, pp. 78–79; also Selgin 1988, chap. 5).

[15]For representative studies of the experience with free banking, see White (1989, p. 369, n. 3). On the question of stability, see Rockoff (1974), Rolnick and Weber (1986), and White (1986).

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dream up cases in which change in the price level would be preferable should have their noses rubbed in that question."

No monetary system is perfect, including free banking.[16] It is an illusion to think that a money of perfectly stable value is possible; it may not even be desirable—for example, a gently falling price level would increase the real value of money and, if expected, would not disrupt market exchange or interfere with individual planning. (Downward price rigidity, however, is an argument in favor of long-run price stability.) A well-defined unit of account, meanwhile, is both possible and desirable. The objective should be to search for a monetary regime that (1) reflects freedom of choice, (2) provides self-regulating market forces to discipline the money-supply process, and (3) allows changes in the quantity of money to respond to changes in the demand for money without disrupting economic life.

The classical gold standard had a well-defined unit of account and operated to provide long-run price stability, but that system did not prevent monetary disequilibrium, and convertibility was suspended whenever politically expedient. A broad-based commodity regime would improve on the gold standard, as would a nondiscretionary monetary rule that was effectively enforced. The problem is that one has little confidence in the enforcers and even less in central bankers who have an incentive to increase their power and prestige by reverting to discretion. That is why F. A. Hayek (1976, 1978) came to the conclusion that the only way to get sound money would be to abolish the government's monopoly of money and make the transition to a system of competing private currencies—a monetary order that would be forecast-free and discretion-free.

Conclusion

Under central banking and discretionary monetary policy, the information requirements for monetary stability are impossible to meet, and there is little incentive for central bankers to subject themselves to binding monetary rules (see Brunner 1985, esp. pp. 20–21). Better macro-forecasting is unlikely to evolve or to solve the information problem, and the incentive problem would remain. A move to nondiscretionary monetary rules—such as a "zero inflation" rule, a constant money-growth rule, or a nominal GNP rule—would improve monetary policy, but such schemes would retain an arbitrarily defined unit of account in the form of government fiat money. Only a move from a government to a nongovernment monetary regime

[16]For a discussion of the shortcomings of free banking, see Selgin (1988, p. 174).

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would anchor the real value of money to market forces, provide a tendency toward monetary equilibrium, and eliminate the need for macro-forecasting and government-driven monetary policy.

Much work remains to be done in evaluating alternative monetary regimes. Recent work by McCallum (1988, 1989, 1990) has shown that his adaptive monetary rule is robust when tested against alternative monetary rules, and is more robust than a stable price-level rule. But his simulations do not capture the practical problems with implementing alternative monetary rules, and he has no way of testing the robustness of free-banking systems.

Since McCallum is uncertain how to specify a quantitative model for testing the robustness of free-banking schemes and since he does not think such schemes are politically feasible at present, he offers his stable nominal-income scheme as a "modest strategy" to improve on the current fiat money regime. Nevertheless, he emphasizes that it is important to consider more radical reforms such as those envisioned by proponents of free banking, because "they stimulate us—indeed, force us—to think about some critical issues in monetary theory that tend to be neglected in more mundane discussions" (McCallum 1989, pp. 343–44).

The fact that it is not possible "to formulate a relevant quantitative model" for free-market monetary regimes, such as the Yeager-Greenfield proposal, "or to estimate the associated shock variances" (as pointed out by McCallum 1989, p. 343), may be a red herring. The possibility of modeling a monetary regime may be less important than the consistency of the regime with the self-regulating forces of the market and with individual freedom, and the likelihood that the free-market monetary order will tend to bring about monetary equilibrium. The market's feedback mechanism of profit and loss, which would discipline private money producers, and the depoliticization of monetary institutions, which would occur if free-market money replaced central banking, are important institutional considerations in selecting alternative monetary regimes—whether or not they can be "modeled."

Unlike central banking, a free-banking system would evolve spontaneously in response to new technology and changes in consumers' preferences. Although the exact nature of institutional innovation within a free-banking system is unpredictable, one can predict the system's operating principles and its plausible outcomes.[17] If the

[17]For a discussion of the types of monetary institutions that might evolve in a free-market environment and for references to the relevant literature on free-market monetary institutions, see White (1989).

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key feature of free banking is that "economic forces reward bankers who make decisions consistent with the maintenance of monetary equilibrium (and minimization of costs) and punish bankers who make decisions inconsistent with these goals," then "the consequences of free banking are predictable" (Selgin 1988, pp. 174–75; cf. Hayek 1987, p. 383).

Thus, although it may not be possible to formulate a quantitative model for a free-market monetary regime, one can derive some implications about the behavioral changes that could be expected in moving from government fiat money to nongovernment money. As Selgin (1988, p. 175) notes,

> The environment it [free banking] produces is favorable to entrepreneurial decision-making and to the undertaking of ventures expected to yield their fruits through long periods. Nothing of the sort can be said of regulated, centralized systems of money supply. This is true not only because those in charge of a centralized system cannot have the information necessary for stability . . . but also because stability is simply not in the interest of those in charge.

The upheaval in macroeconomics in the past several decades, the dismal record of macro-forecasting, and the failure of discretionary monetary policy to achieve long-run price stability provide a challenge and an opportunity to explore alternative monetary regimes and to move toward forecast-free monetary institutions. It is important to consider the implications of making the transition from a centrally planned monetary order to a free-market monetary order—even if free banking and competing private currencies may not yet be politically feasible—because we gain a better understanding of the probable consequences of alternative monetary regimes.

A move toward a nondiscretionary monetary rule, which is largely independent of the macro-forecasting requirements of the present discretionary regime, should be considered as a serious alternative. Friedman's proposal for freezing the monetary base and allowing parallel competing private currencies, for example, would open the door for further evolution of the monetary system. Without such an evolution, we will be stuck with the status quo of central banking and government fiat money. And without the freedom of choice in currency, we will lose the opportunity of having good private money drive out depreciating government money.[18] Indeed, both theory and experience seem to support Hayek's contention that "if we ever

[18]On the process by which good money (money of stable value) would drive out bad money under free banking and flexible exchange rates, see Hayek (1978, pp. 39, 118); also Hayek (1987, p. 387). And on the benefits of "choice in currency," see Hayek (1976, pp. 19-22).

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again are going to have sound money, it will not come from government; it will be issued by private enterprise" (Hayek 1987, p. 383).

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The Art of Forecasting: From Ancient to Modern Times

Donald N. McCloskey

> What men have seen they know;/ But what shall come hereafter/ No man before the event can see,/ Nor what end waits for him.
>
> —Sophocles

> God hides the end of future time in misty night,/ And laughs if mortal worries at what's right.
>
> —Horace

> No, Virgil, no;/ Not even the first of Romans can learn/ His Roman history in the future tense,/ Not even to serve your political turn;/ Hindsight as foresight makes no sense.
>
> —W. H. Auden

Forecasting and Magic

One could take the view that what people had to say about forecasting before the application of matrix inversion is irrelevant. One could believe that our new statistical techniques put us in a different class of forecasts than the ancients—best linear unbiased, surely, and maybe something better. But one doubts it. One doubts that the mere presence of mathematics is enough to change the old pattern.

People have always wanted to forecast the future. In trying to do so they have probably revealed more about themselves than about the future, and the revelation may be of permanent value. In other words, one can learn from the long history of forecasting a little about why people do it and whether it has been good for them, at least if the history exhibits stationarity. The most technically muscular

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forecaster in Washington shares something with a Roman examiner of entrails. The two differ in the thick middle part of their arguments, where the one will discuss the Yule-Walker equation and the other the clefts in the liver of a sacrificial ox. But they are more similar at the ends, in the thinly argued justification for prediction and control.

Around 1600 in England it was reported that "among the common people he is not adjudged any scholar at all, unless he can tell men's horoscopes, cast out devils, or hath some skills in soothsaying" (Thomas 1971, p. 227). The press and public nowadays treat the economist as a soothsayer (if a dubious one), to the point of thinking that economics is directed mainly at forecasting.

But the forecasting of human events—which, of course, is not the main business of economics—has always been magical. In human affairs there is wisdom but not magical omniscience. "The unscrawled fores the future casts," sang the American poet and businessman Wallace Stevens (1972, p. 209), is "damned hoobla-hoobla-hoobla-how."

Forecasting the future seems at first more scientific and grown-up than the mere casting of spells. But forecasting the future and manipulating it are identically magical. The desire to forecast the future and the desire to change it are two sides of the same desire. The one forecasts the future from the flights of birds or the size of the eigenvalues, and is armed by the forecast to prevent evil. The other, less prestigious because more private, already knows the future evil, and arms itself with magic spells and prudent economic policies to head it off.

Real magic claims to have solved scarcity. It leaps over the constraints of the world. If you desire a ride to Baghdad, here is a magic carpet; it you desire your enemy dead, here is a magic doll; if you desire unlimited riches, here is a forecast of interest rates. The magic leaps outside the production possibilities. The "fiat" (the "let it be") in a spell is the desire to get outside what is ordinarily possible.

People desire to avoid heavy investments at the turning point of the business cycle, or they desire to avoid the bad luck from breaking a mirror. Magic that avoids evil is called "apotropaic." One desires an effortless forecasting of avoidable evil. The hiring of economists by politicians and businesspeople who are otherwise hard-nosed is apotropaic.

Children desire that avoiding evil or knowing the future were not so full of effort, a desire that fathers the thought. Small children think that if they wish hard enough it will be so. As Marcel Mauss ([1902–03] 1972, p. 63) noted in his old classic on the sociology of magic, "between a wish and its fulfillment there is, in magic, no

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gap." Small children believe in the omnipotence of thought because they have not yet distinguished their private dreams from the collective dreams we name reality. Adults who cannot make such distinctions are said to be mad.

In *Ancient Egyptian Magic,* Bob Brier (1980, p. 117) describes the pharaoh before a sea battle sinking toy models of enemy ships in his bathtub. Real magic, as distinct from parlor tricks, depends commonly on such a metonymy, taking a thing associated with X as *being* X. It re-presents what the magician wishes to happen. The pharaoh reminds one of economic advisers representing an economy with a model, then sinking the deficit in the bathtub.

Cicero on the Art of Forecasting

The ancient techniques of prediction and control remind one repeatedly of the modern methods. According to Pease and Croon (1970), "Our most important ancient source" is Cicero's *De Divinatione,* written around 45 BC, during an enforced absence from politics. "Divination" is an all-inclusive term in Cicero (*Div.*, II, lxiii, 130): "The power to see, understand, and explain premonitory signs given to men by the gods." Cicero's book is divided into two parts, the first a speech notionally by his brother, Quintus, making the case for divination and drawing on the stoic philosopher Posidonius (Posidonius' works survive only in fragments); and the second Cicero's devastating reply. It makes uncomfortable reading for a modern forecaster.

Artificial and Natural Divination

The techniques of divination were divided into the artificial (*artifices* or inductive), based on observation and open to anyone competently trained, as distinct from the natural (*naturalia* or intuitive), based on dreams and revelation by god-possessed seers. One subdivision of the artificial was divination from living things, such as the behavior of birds (carried out by officials known as augurs or auspices—hence, "under the auspices of someone," it being important that the someone be a proper official), the entrails of sacrificial animals (haruspicy), or merely human omens, such as a sneeze at a critical moment. The other subdivision of the artificial was divination from lifeless things, such as lots (cleromancy, which resembles statistical methods), books (Amish priests are still chosen by *sortes Biblicae*), weather anomalies (*monstra,* the ancestors of sun spots), and, above all, astrology (associated in the classical mind with the Chaldeans and Babylonians, called on account of their numerical obsessions *mathematici*).

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Natural divination comes from within, as in the Greek word "enthusiasm" (god-possessed). As Quintus describes it in Cicero's dialogue (*Div.*, I, xviii, 34): "Those [artificial] diviners employ art, who, having learned the known by observation, seek the unknown by deduction. On the other hand those do without art [the natural diviners] who, unaided by reason or deduction or by signs which have been observed and recorded, forecast the future while under the influence of mental excitement." Dreams, drug-induced trances, calling spirits from the vasty deep (necromancy), and the like had, oddly, greater prestige and plausibility in the ancient world than did the artificial (*Div.*, I, iii, 5). The word "*ars*" or "art," means Greek "*techne*," that is, "technology"—methodical and trained skill. It was better to be directly inspired by the gods than to laboriously accumulate and interpret the data.

Theory-Free Forecasting

The method of theory-free forecasting—leading indicators or Box-Jenkins analysis—is outlined by Quintus (*Div.*, I, xlix, 109): "In every field of inquiry a great length of time employed in continued observation begets an extraordinary fund of knowledge, which may be acquired even without the intervention or inspiration of the gods, since repeated observation makes it clear what effect follows any given cause, and what sign precedes any given event." Of observing crows, "Such signs as these have been observed for an unlimited time, and the results have been checked and recorded. Moreover, there is nothing which length of time cannot accomplish and attain when aided by memory to receive and records to preserve" (*Div.*, I, vi, 12). Or again, "I see, which is enough; why it is possible I do not know" (*Div.*, I, x, 16). And later, "They may not discern the very causes [read "the structure"], but nonetheless discern the tokens of the causes" (*Div.*, I, lvi, 127). This sort of divination, Quintus says, is derived from Fate, that is, from the connectedness of things (*Div.*, I, lvi, 128). The Babylonians claimed to have kept records of horoscopes for N = 470,000 years, enough degrees of freedom for most purposes (*Div.*, I, xviii, 36; II, xlvi, 97).

In other words, the modern distinction of theory-free and structural models of forecasting repeats the ancient distinction between artificial and natural divination. The artificial relies on the external signs, observed 470,000 times if one is fortunate in one's data set. The natural relies on internal intuitions, in the form of specifications of the structure or the other constraints of an economic theory not itself derived from the signs. The "tender loving care" that modern forecasters routinely apply to their (artificial) forecasts, in order that

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judgment may have its proper place, is again natural and god-inspired.

Combining the two, Box-Jenkins with structural estimation, was prudent among the ancients as among the moderns. Oedipus invites Teiresias to save the city with every device of prophecy: "Do not begrudge us oracles from birds,/ or any other way of prophecy/ within your skill; save yourself and the city,/ save me" (Sophocles, *Oedipus the King*, 1, 310ff).

Ancient and Modern Skepticism

The attacks on ancient forecasters, and their defenses, have also a modern sound. Astrologers (sometimes including "sorcerers, philosophers, and other diviners") were ordered expelled temporarily from Italy on nine occasions between 139 BC and 93 AD (Scullard 1970). Augustus outlawed private casting of horoscopes in 11 AD—the problem was the treasonable possibility that someone would forecast the death of the emperor, an argument for censorship that would probably appeal to the Defense Department and certainly would appeal to Saddam Hussein. Diocletian (emperor 284–305) banned all divination, "*ars mathematica damnabilis.*" The Christian emperor Constantius II made divination a capital offense in 357 (the law had to be reenacted in 373 and 409, not instantly obeyed). By the 14th century Dante (*Inferno* 20: 38) provided a special place in hell for any "whose glance too far before him ranged," their heads twisted back-to-front.

Cicero attacks haruspices on the same grounds that modern forecasters are attacked, namely, that they disagree (*Div.*, II, xii, 28; compare for auspices II, xxxix, 81): "*An haec inter se haruspices Etrusci, Elii, Aegyptii, Poeni contulerunt?*"—"On these matters among themselves are the haruspices of Etruria, Elis, Egypt, and Carthage agreed?" Are the economic forecasters of MIT, Chicago, and Washington agreed on when the recession will end?

Any fault in forecasting, however, lies in the interpreter, not the signs or the method: *male coniecta maleque falsa sunt non rerum vitio, sed interpretum inscientia.* (The word "*vitium,*" here "*vitio,*" means "sin" but in particular means a failure to perform the ceremony correctly: an error of specification, or even of keypunching).

Even the statistical *vitia* of modern times find ancient parallel. Rigor about sampling errors and vagueness about errors of specification (compare Leamer 1983) or of conditioning (that is, the future conditions X derived from somewhere else put into the model of Y) are a feature of both ancient and modern practice. So is the turning

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over of decisions to the technicians, contrary to decision theory (the oracle at Delphi was careful to keep informed about current events, the better to judge what losses the clients would suffer if they acted on a wrong forecast). So are the use and misuse of significance tests. Quintus (*Div., I, xii, 23*) argues thus, perfectly cogently: " 'Mere accidents,' you say. Now, really, is that so? Can anything be an 'accident' which bears on itself every mark of truth? Four dice are cast and a Venus throw [four different numbers] results—that is chance; but do you think it would be chance, too, if in one hundred casts you made one hundred Venus throws?" One is reminded of the 18th-century argument that the closeness of the orbital planes of the planets implies a common origin (on grounds that such a peculiar arrangement is unlikely)—and the reply that, after all, *any* particular arrangement is equally unlikely. Cicero himself gives an answer (*Div.*, II, lix, 121): "Then are we, like fools, to prefer to say that it happened in the direction of Venus rather than by chance?"

Well, what of it? This: that the difficulties with forecasting are ancient. The root difficulty with forecasting is what is known in ancient rhetoric as "the circumstantial ad hominem" argument. One argues against the opponent on account of his embarrassing circumstances. Though the argument has no place in formal logic (so much the worse for formal logic; compare Walton 1985), we use it daily. A lawyer arguing for the Tobacco Institute had better be seen smoking in the hallway during the recess. That our teenage daughter has a strong self-interest in not revealing that she took the car at 2:00 A.M. will properly cast doubt on her protestations of innocence. It is relevant to the assessment of a Mafia informer that he is by profession a liar and a cheat.

The Roman poet Ennius was only one among many to use the circumstantial ad hominem to sneer at forecasters, "who for themselves the path do not know, yet for others show the way" (*"qui sibi semitam non sapiunt, alteri monstrant viam,"* quoted in *Div.*, I, lviii, 132). Socrates attacked the other sophists for charging for their advice, to which the sophists retorted that at least their advice was worth something (Isocrates, *Soph.* 2f; compare 7; cited in Jaeger 1944, III, p. 57). It was said that the philosopher Thales of Miletus made a fortune buying up the olive crop in his district (*Div.*, I, xlix, 111), "in order to show that even a philosopher if he sees fit is able to make money." And yet most of these philosophers and seers, like modern professors of economics, were not rich.

An economist who claims to know what is going to happen to the price of corn is claiming to know how to make money. Many models printed for free in the journals of agricultural economics imply fore-

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knowledge of the price of corn. With a little borrowing on the equity of his home or his reputation for sobriety, the agricultural economist can make enormous sums. If an agricultural economist could forecast the price of corn better than the futures market, he would be rich. Yet he does not put his money where his mouth is. He is not rich. It follows that he is not so smart. The same holds for all diviners, macroeconomists, astrologers, urban economists, and technical elves.

The argument applies only to *profitable* forecasts. A forecast of quantities is normally not profitable. An agricultural economist who knows the tonnage of next year's carryover of soybeans does not by that fact possess information valuable on the commodity exchanges. But profitable forecasts (at any rate if they were believable) are more common than most economists seem to realize. True, the average economist realizes that he cannot reasonably expect a course in econometrics to give him tips on the stock exchange. But any economic model that forecasts the price of anything correlated with an asset price is just as good as a hot tip concerning a buyout of General Dynamics. Many economic models forecast such price-correlated things. The turning point of the business cycle, for example, is correlated with the prices of business property and corporate stocks and government bonds. To put it more generally, the price change of any storable good, storable service, or financial or real asset (assets being storable by definition) should be unpredictable from any "artificial" (that is, mechanical) procedure, at least outside the "gold points" set by the normal rate of return and the premium for uninsurable risk. Eugene Fama stated the condition long ago for financial markets. The point here is merely that it had better apply to all assets, or else the forecaster is claiming to be very smart indeed. He who is so smart claims a Faustian knowledge, "Whose deepness doth entice such forward wits/ To practice more than heavenly power permits."

Economists' Objections

Most economists are not comfortable with such ancient skepticism. They have objections. It may be objected that the profitmaking is risky and that professors of economics are cautious. Therefore, they do not put their money where their mouths are, even though their mouths are working fine. Even a worldly philosopher, if he sees fit, is able to make money. The objection has the problem that the bet on the price of corn can be hedged, which is insurance. It is no bet. Someone who can outsmart the market on average, even a little, can

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make a lot of money simply and safely. No wonder: The opportunity to buy corn low and sell high, like the right to run a TV station in the 1960s or to import Toyotas in the 1980s, is like finding a $500 bill any time you want.

It may be objected that profitmaking is complicated, and that professors of economics are elaborately trained experts in the complexities. Therefore, the profit is not available to just anyone, only to them. The wizards earn merely what they are worth, the normal return on years of studying wizardry. This objection, too, has little to commend it. The wizards are telling us about the future price of corn or bonds or housing at cocktail parties and in the newspaper, free. Why are they handing over to John Doe their just reward for going to wizard school? And the wizardry claimed is systematic, formulaic, and, when you come right down to it, pretty simple. It involves the fitting of a few hyperplanes to scatters of points. Take a course in economic statistics, the promise goes, and become able to forecast the future in profitable ways. The promise is hard to believe.

Ordinary secrets and routine advice do flow from economic forecasting, and doubtless economists earn their keep (less by forecasting, one would think, than by now-casting). Unlimited wealth, however, cannot be expected to flow from a book or even from many years of concentrated study in economics. Compared to unlimited wealth, many years of study is like the trivial cost of reaching down to pick up a quarter on the ground (Manhattanites, it has been found by experiment, will not stoop for less). If someone knows a scholarly formula for forecasting the price of corn or the level of interest rates on the turning point of the business cycle, it would already have been exploited.

It may be objected that sophisticated people do, in fact, buy stock market advice. An economist (and only an economist) would conclude that something of value had been bought. A reply has been suggested by James Burk (1988), a sociologist and former stockbroker, who found that the advice-giving industry sprang from legal decisions early in the century. The courts began to decide that the trustee of a pension fund or of a child's inheritance could be held liable for bad investing if he did not take advice. The effect would have been the same had the courts decided that prudent men should consult Ouija boards or the flights of birds. It was so at Rome: A consul who ignored the advice of the college of augurs was liable to prosecution after retirement. America decided through its judges that an industry giving advice on the stock market should come into existence, whether or not it was worthless. It did, and was. (Europe

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is not similarly blessed, because the law is different.) And, of course, in other ways popularity is no guide. Cicero (*Div.*, II, xxxix, 81) mocks his brother's argument from popularity: " 'But,' you say, 'all kings, peoples, and nations employ auspices.' As if there were anything so absolutely common as want of sense!"

It may be objected that, after all, a great deal of money is made in the stock market. So also is money made at the track in Miami. Grandfather Stueland was offered Radio Corporation of America stock in the early 1920s, and regretted later that he had invested in Stueland Electric instead. Some people did buy RCA: They must have known. But that some people win with the stockbroker or the $100 window at Hialeah does not mean that they were justified in their true belief. They could have won by luck rather than by a justifying technique. People win at slot machines, too, but cannot tell how, because they use no justifiable, inscribable, bookable technique. And even if some people *do* know they will win (God appears to them in a dream and tells them, in the style of natural divination; or they have genuine inside knowledge, in the style of artificial divination), there is no way for the common pigeon to know that these alleged experts know. Why would they be telling us?

It may be objected, at last, that the economist or other seer in the stock or bond or housing market does not have access to the big loans to make big money. Yet consortiums do, and if the wisdom comes simply from being an economist, consortiums ought to be simple to assemble. A consortium of famous economists at Stanford and the University of Chicago in the early 1970s believed that interest rates, which were then at shocking, unprecedented highs (6, 6.5, my Lord, even 7.5 percent), just had to come down. The economists complained at lunch that their bankers would not loan them money to exploit this sure thing. But in the event, sadly, the bankers were right. Interest rates did not fall; they rose. The consortium of economists, relying on its collective expertise in forecasting, lost its collective shirt.

The routine is the usual one. I myself have lost a shirt or two on real estate deals that were bound to succeed and on a consortium of economists speculating in the foreign exchanges. From John Maynard Keynes (who lost money regularly before breakfast but had a Cambridge college backing him up) and Irving Fisher (who reduced Yale's endowment to half of Harvard's by touting stocks in 1928) down to the latest scheme of some economist to make money from mathematical models of gold speculation, economists have not earned the confidence of bankers. As it was put by Paul Samuelson ([1982] 1986, p. 541), who is in a position to know, "It's a mugs game

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for a dentist—or an associate professor of econometrics—to think that he and the telephone can have an edge over those who count the cocoa pods in Africa and follow the minute-by-minute arrival of new information."

The best-known counter-example among economists is said to be the late Otto Eckstein, a superb economist who had much natural, god-inspired sense and who extended the large-scale econometric model into commercial use. He built Data Resources into a company with revenues of $84 million in 1984. But Data Resources did not use its own forecasts of prices and interest rates to speculate. It sold them to others, mainly to companies who wanted a myth of knowledge to comfort them in uncertainty and to answer wrathful stockholders: "We took the best advice." If Data Resources had believed its own forecasts to the extent of speculating on them, and was correct in its belief, then it could have become fabulously richer than it was. To say that Eckstein or Samuelson or other honest purveyors of economic tips became, in fact, a little bit rich does not say that they are so smart. Eckstein and Samuelson (and Louis Rukeyser of Wall Street and Hot Horse Herbie of Broadway) became rich by *selling* advice, in the form of models and statistical equations and other charming talk, not by using it.

The Need for Modesty

Cato the Elder wondered that the haruspices, who examined livers in Rome with an expertise approaching the econometric, did not laugh on meeting one another (quoted by Cicero, *Div.*, II, xxiv, 52). Economists know lots of similar gags about their inability to forecast profitably: Forecasting is very difficult, especially if it is about the future (the physicist Niels Bohr is supposed to have originated this one); an economist is an expert who can tell you tomorrow why the thing he forecasted yesterday did not happen today; the best I can hope in a forecast is to be intelligently wrong or fortunately right.

Yet one must not get carried away. No one doubts that a well-informed economist can tell you a thing or two about the future, mainly from knowing the present well. As Robert Solow (1982) remarked about the forecasts from Eckstein's Data Resources, "Every month it provides an orderly description of the data, organized in such a way that one's attention is called to events that seem to conform with a reasonable person's understanding of the economy." Ancient skepticism does not undermine forecasts that offer little or no profit. A forecast makes no profit if it is commonplace or if does not offer a way to buy low and sell high. Forecasting that

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the national income will not fall to zero next year is no more profitable than forecasting that the sun will rise tomorrow.

Other people view economists as social weather forecasters. Economists are not so happy with the analogy, since they know they are not so smart. Weather forecasters and price forecasters could both earn a lot of money on a good forecast if they could keep it secret. A study by Victor Zarnowitz and Geoffrey Moore (1982) showed that leading indicators, invented by Moore, can indeed forecast business cycle peaks—but with leads, alas, ranging from 1 to 19 months. "The economists are generally right in their predictions," Sidney Webb said once, "but generally a good deal out in their dates." Forecasting the end of prosperity as coming somewhere in the next 19 months is a little better than saying that if it is August then southern Florida has a fair chance after a while of getting a hurricane. Yet it is not so smart that the economic forecaster could retire to Miami. It is not good enough to be profitable; and if it were, it would be discounted already.

There are other ways than the circumstantial ad hominem of getting to the same doubt that economists can forecast. For one thing, unlike humans, hurricanes are not listening. Humans react to economic forecasts in ways that dampen or magnify the forecasts. It would be as though the hurricane presently north of Cuba reacted to a forecast that tomorrow it was going to move to Miami by saying, "Hmm: I'd better turn around and go to Haiti instead." People are not so stupid that they are easy to surprise; if they are not easy to surprise, then the economy is not easy to manipulate, and its manipulators would not be rich.

Further and more deeply, the equations of fluid dynamics applicable to the weather do not include an equation that rules out cheap but profitable forecasts. Economic models do. A person who was smart enough to know the solutions to the economic equations would be rich, unless profitable solutions were already anticipated and discounted by the model. Such solutions should be discounted. If the model is a widely available piece of information, or if its essence were embodied in a widely held judgment, it would be useless for making anyone rich. Wise in retrospect, maybe; rich in prospect, no.

The late-modern economist is modest about profitworthy detail, the detail from which he could buy low and sell high. He must be modest especially about the proud claim of economics in the 1960s, the claim to fine-tune the economy, making detailed adjustments to money and taxes in order to offset a depression just around the corner. As economists realize now after much tragedy sprung from hubris, if an economist could see around the corner he would be rich. Fine-

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tuning violates the ancient and justified circumstantial ad hominem against forecasters. A fine-tuner would see dozens of opportunities for personal enrichment. The economists go on relating impossibly detailed scenarios into the microphones of television reporters, but in their hearts they know they are wrong.

The argument recommends intellectual modesty in the economic forecaster, if he does not want people to laugh on meeting him. Hubris will need divine protection. Xenophon (*Mem.*, I, 1, 7) reported Socrates saying:

> Those who intend to manage [*oikesein*, from which "economics"] houses or cities well are in need of divination. For the craft of carpenter . . . or economics [*oikonomikon*] . . . may be learned . . . ; but the greatest of these matters the gods reserve to themselves. . . . If anyone supposes that these [divinations] are not beyond reason, and nothing in them beyond our judgment, he is himself beyond reason.

Socrates could turn to the oracles for divine supplementation of a craft. We have lost today the favor of the gods, and books on econometric technique will not assuage our woe.

The Limits of Forecasting

Notice that the reason it is difficult to forecast is not merely that humans are too complicated or too changeable or too free. The humanistic criticisms of social science may be true but they are not telling; they are easy to make and easy to answer. The scientist answers, "Give us the money and we will finish the job." If humans are "ultimately" free, considered as individuals, they still can be forecasted on average and in the mass. And if human masses are complex, they still can be forecasted with another million dollars and another model. So long as humans are to be viewed as molecules bouncing against each other, the problem is merely to get the mathematics right. It is said that forecasting human beings is bound to be more complicated than forecasting planets or pigeons, but that is not true. It depends on what you are trying to forecast. The heart beat of a human is easier to forecast than the twitching of the 67th feather from the pigeon's tail. It is a matter of how ambitious the forecast is. The "simple" problem of space flight, "merely" an application of Newton's laws, requires days of computation at high speed if the ambition is to put a rocket precisely *there* on Mars. For a given ambition the complexity is only a matter of computer time.

The circumstantial ad hominem puts more fundamental limits on what we humans can say about ourselves. It puts a limit on mechanical

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models of human behavior. It does not make the mechanical models useless for interesting history or routine forecasting; it just makes them useless for gaining an edge about the future. The various "solutions" of bargaining problems have this flaw: that if the economist knew the solution, then so would the players, which would make the solution valueless. The Turing machine that could forecast the next move of a competitor would sell for a lot of money. If Turing machines are cheap, no one can get rich by using them to outsmart others.

In the opening lines of *Faust,* before the Doctor has turned in vexation to magic, he laments that "I see that we can know nothing!/ It nearly breaks my heart." He immediately amends this sweeping skepticism, for the skepticism about forecasting does not imply literally that we can know nothing but merely, as he then complains on behalf of his fellow men, that he can know nothing, as he puts it disingenuously, *to better Mankind.* I'm from the forecasting agency and I'm here to better you. On further reflection he comes to the nub: His studies, damn them, have taught him nothing that betters *Herr Dr. Faust,* this very example of Mankind. "And *I* have neither property nor money,/ Nor honor and glory in the world:/ No dog should go on living so." There lies the tragedy, at the impossibility of forecast that would be profitable to Faust himself. He seeks the ability to forecast for personal profit (which in due course he obtains, though not for free) and gets his soul's fill of property and money.

Lacking the Devil's bargain, science cannot forecast itself. The paradox shows up in economics because economics so plainly must apply to itself, if it is so smart. But the paradox applies to any foreknowledge of new knowledge. The impossibility of self-forecasting has become commonplace in philosophy. You do not know today what you will decide tomorrow, unless you have already decided it, in which case it is not tomorrow but today that you decide it.

"Prescience" is an oxymoron, like cheap fortunes: "Pre-science" is knowing before one knows. Prescience is required for central planning of science as much as of the economy. Karl Popper and Alasdair MacIntyre among others have pointed out that knowing the future of science requires knowing the science of the future. It is not to be done. MacIntyre notes that the unpredictability of mathematical innovation is a rigorous case, resting on theorems concerning the incompleteness of arithmetic and the incalculability of certain expressions, proven by Gödel, Church, and Turing in the 1930s. And "if the future of mathematics is unpredictable, so is a great deal else" (MacIntyre 1981, p. 90). If someone claims to know what method or

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lack of method would yield good science, why is he not scientifically rich?

The distinction between routine forecasts and startling and profitable divination is analogous to the distinction between routine cooking and the profitable art of three-star cookery. In the peculiar little dialogue, the *Ion*, Plato/Socrates lampoons Ion the performing artist who imagines he *knows* something. It is significant that to mock Ion's claim to knowledge Socrates uses the example of divining. Allan Bloom (1970, p. 57) once remarked of the passage:

> If divining is to be considered an art, it is strange in that it must profess to know the intentions of the gods; as an art, it would, in a sense, seem to presuppose that the free, elusive gods are shackled down by the bonds of intelligible necessity. Divining partakes of the rational dignity of the arts while supposing a world ruled by divine beings who are beyond the grasp of the arts.

As Plato and the skeptic about forecasting would say, the claim of divining to be an art, Greek *techne*, mere bookable craft, is absurd.

Plato, therefore, wished to cage poetry, the god-possession that flatters men to think they know more than does the honest artisan, a technician in every sense. The followers of Plato down to the present age of technique are enamored of knowledge as *techne*, a craft written down in books. They propose to cast books lacking such craft into the flames, as poetry and pretense, mere sophistry and illusion. The trouble is that their version of the fully rational life, the bookable final rules for language games, requires unusual forecasts. And in human affairs a forecast beyond what earns merely usual returns is impossible, except by entrepreneurs, idiot savants, *auteurs*, and other prodigies of tacit knowledge. The notion that bookable knowledge can guide the world through its difficult moments, like the notion that central planning can guide an economy, is self-contradictory. If the philosopher kings and central planners were so smart they would be rich.[1]

The Market for Forecasting/Magic

Nothing I have said implies that the project of acquiring systematic knowledge about the economy is worthless. Inside the margin, as economists say, it is worthful. The world runs on little else. Everyone needs to know how to write an alphabet, though it took a Phoenician

[1]As indeed they are, for a reason other than their ability to forecast. They live in a world ever hopeful that procedure, mechanism, calculation, bureaucracy, MBA degrees, and other social *techne* will keep us warm and safe. It will not, though the world is willing to pay handsomely for the illusion.

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genius to think it up and make his fortune. No one afterwards, though, can expect to make a fortune by knowing the ABCs.

An economist examining the business world is like a critic examining the art world. Economists and other human scientists can reflect intelligently on present conditions and can tell useful stories about the past. These produce wisdom, which permits broad, conditional "forecasts." Some are obvious; some require an economist; but none is a machine for achieving fame or riches.

The argument is merely that at the margin (where supernormal profits and reputations for genius are being made) the observer's knowledge is not the same as the doer's, the critic is no improvement as artist over the artist, the model of the future is no substitute for the entrepreneur's god-possessed hunch. The critics become ridiculous only when they confuse speaking well about the past with doing well in the future. Critics of art and literature stopped being ridiculous this way a long time ago. It would be a fine thing if critics of society would join them in their modest and sober sophistication.

The economics itself says that desires cannot be granted easily and that magic is without practical efficacy. Economics is postmagical, the way the ancient poets and philosophers were. Yet the economists give in to the demands of their clients. The demand for prophecy is insistent. People believe that someone has the gift and are willing to listen to Nostradamus (the sales of his prophecies from Walden Books increased 400 percent following the outbreak of the new Persian Wars). When Teiresias tries to get out of delivering his terrible prophecy, Oedipus chides him angrily: "You'd rob us/ of this your gift of prophecy? You talk/ as one who had no care for law nor love/ for Thebes who reared you" (Sophocles, *Oedipus the King*, 322–24).

J. S. Armstrong (1978, p. 399) has proposed a "rain dance" theory of why expensive forecasters get hired: "The rain dance has something for everybody. The dancer gets paid. The clients get to watch a good dance. The decision-maker gets to shift the problem onto someone else in a socially acceptable way. (Who can blame him? He hired the best dancer in the business.)"

More economistically one could argue that people will not put a high value on what is cheap. The forecasts must be more expensive than merely snapping one's fingers, or else it will not be accounted magical. To deal with uncertainty, we need magic all the time. But the magic cannot be too easy. A magical spell that is valuable is believed; and, therefore, one way to get it believed is to make it expensive, boldly asserting its value. ESSL Corp. (P.O. Box 66054, Los Angeles, CA 90066) sells the most expensive of six programs available from various companies to guess the next numbers on the

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lottery. The program sells not for its cost of production (a few dollars) or for its value in use (nothing) but for the persuasively substantial price of $59.95. Psychoanalysts require that the patient pay, because otherwise the therapy will not work. The patient has to make a sacrifice to get well. Divintel, one of several French astrological companies serving business clients, sells a consultation for FFr350 ($70). According to a survey by HEC, a French business school, one-tenth of French businesses use the service.

In other words—and this is the essential point—magic has to be expensive. The economic idea is rent seeking. Magic promises something for nothing. We clever moderns know, however, that magic does not work. If magic is to survive, it must be made expensive, or else people would complain that they snapped their fingers but still mistook the future. The rhetoric of magic demands that the magic be difficult to perform, or else its failure to work will be too evident. Magic promises profit, the same way an economic forecast does. Therefore, the business of magic will attract entry, at length driving down its profit. At length the costs of making the last bit of magic will equal its value in free rides to Baghdad or in daddies conveniently disposed of or in golden opportunities to profit from the coming financial crisis.

When forecasting is not being responsible, it sounds like magic. Magic, like forecasting, is often secret. Mauss ([1902–03] 1972, p. 23) argues that "religious rites are performed openly, in full public view, [but] magical rites are carried out in secret. . . . And even if the magician has to work in public he makes an attempt to dissemble, . . . [to] hide behind simulated or real ecstacies." One can doubt Mauss's assurance that magical and religious ceremony are sharply different, yet agree that secrecy is common for the magical type. The secrecy makes for scarcity, no less than a secret recipe for baking bread or a secret method for casting iron thin enough for pots.

Likewise, magic and the hot econometric tip are often exclusive. "Nobody can become a magician at will; there are qualities which distinguish a magician from the layman" (Mauss [1902–03] 1972, p. 27). Again the economics works to raise the price. The list of candidates for magical powers must be restricted to make it expensive. As Socrates says in his elitist way, "Not everyone is an artisan of names, but only he who keeps in view the name which belongs by nature to each particular thing" (Plato, *Cratylus*, 390E). The expert can see into the mind of God, being "a master of name giving" (389D; here as elsewhere it is hard to know if Plato is joking or not).

The candidate for magician must be unusual in some way. Smiths, barbers, shepherds, foreigners, infidels, primitives, and other

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special, lonely people can become magicians (Mauss [1902–03] 1972, p. 28ff), meaning that one man's tribe is another man's league of sorcerers. The Jews were thus steadily suspected of magic, and the Lapps could sell bags of wind to European sailors (Mauss, p. 32). Specialness, not rarity, is the key—for women were commonly considered magical (because they were excluded from religion and from science), and there are plenty of women. Keith Thomas (1971, p. 274) speaks of the advantage that priests had over ministers in appropriating magic in the 16th and 17th centuries: "Precisely because the Church had its own magic . . . it frowned on that of others. . . . Set apart by his learning, his unique ritual power, and his official virginity, the priest was admirably qualified to be a key figure in the practice of popular magic." A magician learns the language of the spirits, at large cost. He knows the words the gods speak (Mauss, p. 38f; *Cratylus*, 391E; contrast 401A; and the gods loving a joke, 406C). The magician undergoes initiation. Any man can call spirits from the vasty deep, but only for a magician—or an elaborately trained econometric forecaster—will they come.

Above all, magic is often elaborate. It is notoriously so, hoobla-hoobla-hoo, an image of mysterious wisdom won by toil. The rites can last hours or days or weeks, the length of some computer runs. Magic is repetitious, covering every possibility—or else it does not work, since tiny failures to follow the prescription protect the magician from responsibility, the convenient *vitia*. "It is natural for a magician to take refuge behind questions of procedure and technicalities, to protect himself in case of failure in magical prowess" (Mauss [1902–03] 1972, p. 50; compare Thomas, p. 641).

Magical ceremonies are usually more elaborate than religious ceremonies. The scarcity in religion is accomplished by a restricted priesthood and, especially, by the limited efficacy of prayer, which otherwise looks like the fiat in a spell. The Christian sects with less elaborate preparation for their priests expect less from prayer, the one scarcity compensating for the other. The extreme cases are the Amish priests chosen by biblical lot or the Quakers with no priests at all. The sects that think that prayer works cheaply, every time, are regarded by mainstream religion as magical—for example, the cults of saints in the south of Italy or pentecostal sects in the United States.

Magic, then, is childish and anti-economic. Childishly, it gives way to the pressure of desire. Scarcity is wished away. But adults and economists know that scarcity cannot really, truly be wished away. The scarcity must show up somewhere. It does show up in most features of magic. And to the extent that forecasting is, in fact,

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magic, it will exhibit the same signs of rent seeking. The supposed profit arising from the evaded scarcity gets absorbed, competed away. Economics says: At the margin the hoobla-hoo must absorb the incipient profit from being able to take the magician to Baghdad on a carpet, if he could only get the damned thing running.

Economics as a science is a force of acculturation. It says: You cannot get that. The churches of the 16th and 17th centuries, Thomas (1971, p. 278) argues, put "strong emphasis upon the virtues of hard work and application" and "helped create a frame of mind which spurned the cheap solutions offered by magic, not just because they were wicked, but because they were too easy. Man was to earn his bread by the sweat of his brow." Like poetry, and unlike magic, economics in the century afterward dwelt on scarcity. It came to tell that all good things must be scarce in equilibrium; all magical opportunities must be used up. It tells us we must work by the sweat of our brows to achieve our desires. It tells us that we cannot be rich by snapping our fingers. And it tells us that individual morality does not ensure civic morality. Such hard messages would have been perhaps too hard for earlier and less settled times.

Economics is the science of the postmagical age. Far from being unscientific hoobla-hoo, economics is deeply antimagical. It keeps telling us that we cannot do it, that magic will not help. Only the superstitious think that profitable forecasts about human action are easily obtainable. That is why economics, contrary to the common sneer, is not mere magic and hoobla-hoo. Economics itself says that forecasts, like many other desirable things, are scarce. It cannot be easy to know which great empire will fall or when the market will turn. "Doctor Friedman, what's going to happen to interest rates next year?" Hoobla-hoo. Some economists allow themselves to be paid cash money to answer such questions, but they know they cannot. Their very science says so.

Sometimes, however, in its affection for forecasting, economics exhibits a nostalgia for magic. Therein lies the danger. Economics can go wrong and betray its postmagical sophistication by surrendering to the temptation of magic. If, say, poetry surrenders, we are perhaps not seriously damaged—although the poetry then stops performing its maturing function and can even rouse men to magical beliefs in the white man's burden or in some corner of a foreign field that is forever England. Such notions are mischievous enough. But an economics that is nostalgic for magic is radically dangerous.

Social Engineering: The Fatal Attraction

The danger lies in the uses of forecasting, particularly in the comfort it gives to the present state religion. The old state religion of

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Rome was that the gods were in their heaven and all was well with the world, so long as Romans did their ritual duty, consulting whatever signs the gods might send. The ability to forecast the future is plausible on a theistic premise (Cicero, *Div.*, I, xxxviii, 82–83). The gods know; why would they not sometimes tell us? As Quintus says (*Div.*, I, xlvi, 104), "To make light of signs sent by the gods is nothing less than to disbelieve in the existence of the gods."

A sense of public duty, the republican virtue, was central to the Roman state religion. Quintus was suspicious of *qui quaestus causa hariolentur*—those who for profit's sake would prophecy. The forecasters were to give their wisdom to the republic. In early Rome "it was wisely decreed . . . that, of the sons of the chief men, six should be handed over to each of the Etruscan tribes [skilled in such matters] for the study of divination, in order that so important a profession should not, on account of the poverty of its members, be withdrawn from the influence of religion [i.e., state piety], and converted into a means of mercenary gain" (*Div.*, I, xli, 92).

The present state religion is similarly underpinned. It says that civil servants look after the common good, as the Progressives hoped, or at least that men of integrity will do their duty, as the Patricians hoped. It says that the common good can be sought positively, as the New Dealers hoped. And it says that common behavior can be manipulated with ease, as the knights of the Great Society hoped. All these hopes converge (unsupported hope being a theological virtue) to create a faith in prediction and control. "A really splendid and useful thing it is—if only such a faculty exists—since by its means men may approach very near to the power of gods" (*Div.*, I, i, 1).

To do for others through statecraft what they cannot do for themselves is social engineering. The best social engineering seeks positive freedom—freedom from want—and can achieve it if it can forecast accurately, which it often cannot. But in the meantime it is likely to be careless of negative freedom—freedom from control. Frank Knight ([1936] 1947, p. 38) noted the rhetorical contradiction in the idea that we can be helped by forecasting social engineers: "Natural science in the 'prediction-and-control' sense of the laboratory disciplines is relevant to action only for a dictator standing in a one-sided relation of control to a society, which is the negation of liberalism—and of all that liberalism has called morality."

Distrust of forecasting is not mere fatalism. The fatalistic view was well expressed by Jocasta advising Oedipus, "Why should man fear since chance is all in all/ for him, and he can clearly foreknow nothing?" (Sophocles, *Oedipus the King*, 977–78). "Foreknow nothing"

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is too strong, leading in Jocasta's motherly/wifely advice "to live lightly, as one can, unthinkingly." Fatalism anyway is an Old World attitude, ill-fitting with American optimism.

And yet the ancient skepticism against forecasting is the beginning of wisdom. By all means attend to the forecasts of experts, as one must in order to live; but expect little, and trust them not. The beginning of maturity is to know that the "best-laid schemes o' mice and men / Gang aft a-gley." The beginning of responsible freedom is to know that yon expert "birkie, ca'd a lord,/ Wha struts, an' stares, an' a' that; / Tho' hundreds worship at his word, / He's but a coof for a' that."

No expert forecaster is justly subject to the skeptic's sneer who retains a proper modesty about what observation and recording and storytelling can do. We can observe the history of economies or the history of painting, and in retrospect tell a story about how security of commercial property or the analysis of vanishing points made for good things. An expert such as an economist is an expert on the past, and about the future that can be known without divine and profitable possession. Human scientists and critics of human arts, in other words, write history, not prophecy.

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The Limits of Economic Forecasting

Michael D. Bordo

Donald McCloskey cogently argues that both the art of forecasting and the case against it are ancient. The modern art of economic forecasting is compared to ancient divination. There are two types of divination: artificial and natural. Artificial divination makes forecasts using the signs of nature and the heavens. Natural divination relies on dreams and visions. In modern forecasting a two-fold division can also be made—between the mechanical Box-Jenkins type analysis of the pattern of time series and the use of structural models derived from economic theory coupled with the use of judgment.

The case against forecasting is also ancient. Cicero argued, and McCloskey reiterates, that forecasting cannot really have much value, because if forecasters could really tell the future, they would be fabulously wealthy and are not. The reason economists cannot forecast the future of variables such as asset prices from economic models is because if they could do it, others could do it too—the new information would already be discounted. In other words, opportunities for profits will always be exploited; there is no such thing as a free lunch.

Some Questions

If economic theory tells us there is no real advantage to forecasting, then why are economic forecasts paid such heed? The answer is because they satisfy a demand for magic. People want free lunches, and they are willing to pay so-called experts considerable sums of money and other rewards to tell them stories about the future. If, as it turns out, the forecasts are wrong, then the users can blame the expert and the expert could reply, "Who could foresee a war?" Thus, a caste of expert forecasters is created. For forecasting to have value, forecasts must be complicated and understood by only a select few.

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If forecasting were only a form of snake oil, then why should we worry about it any more than the sale of patent medicines or aphrodisiacs? The answer, which McCloskey spells out loud and clear, is that forecasting can become a problem when the government uses it for social engineering. If the government can forecast recessions accurately (i.e., know both the sources of shocks hitting the economy and the pace and timing of their relationship to the economy), then it can prevent them by the appropriate use of its monetary and fiscal tools. The problem with this prescription for fine-tuning, as Milton Friedman ([1951] 1953) pointed out, is that if the forecasts are wrong, then the fine-tuning can actually exacerbate the business cycle. Moreover, as Robert Lucas (1976) made clear, if the government can set up a structural model of the economy and use it to make both policy prescriptions (by simulations) and economic forecasts, so can the public. The public will incorporate the model into their decisionmaking and formation of rational expectations so that the government, even if its forecasts were reasonably correct, could never successfully manipulate the private economy.

Finally, does this mean that economic forecasting has no role? Here McCloskey takes the quite reasonable position that economists have an important role in explaining how the economy works today and how it has worked in the past. With this understanding, economists can make conditional forecasts that the government or private sector can use with a degree of caution.

Some Qualifications

I basically agree with the author's conclusion—forecasting is limited at best. However, I would like to qualify four points and salvage a bit of the case for forecasting.

First, although forecasting is limited, it has value and not just in the sense that anything that sells in the market has value. Forecasting has value in the conditional sense mentioned above. Moreover, it gives the public and the government flexibility. By providing information on how the economy works in the present and has worked in the past, and also in providing information on how past and present variables related to future variables in the past, an economic forecast gives insights on what could occur in the future. As McClosky correctly says, no one knows exactly what will happen in the future, but when an economist gives us information that increases our understanding of what could happen, and given what has happened, we have greater flexibility in making choices. He is correct in saying that an economist who says he knows exactly what will happen,

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because his economic analysis tells him so, is being dishonest. But the economist can still present the menu and then say what his personal hunch is. The user, knowing the economist has not put his money where his mouth is, can then take the advice with a grain of salt—*caveat emptor.*

Second, economic analysis can be used to make qualitative predictions that can then help the policymaker (e.g., if you expand the money supply, inflation will follow; if you freeze prices, shortages will follow). Historical evidence can be used to back up these predictions.

Third, some forecasters can be better than others. The market will rate them, which indeed it already does. The key criterion used today is the forecasters' track record. Presumably, credibility and candor are also important criteria.

Fourth, there is a quibble over whether economists can make fortunes by using their economic knowledge to predict the future. According to McCloskey, entrepreneurs possess hunches—they make gambles based on their forecasts of future opportunities—which are sometimes successful. Economists as a group, however, cannot do that because any opportunities for profit that could be revealed by theory will be arbitraged away. To support his hypothesis, he gives examples of famous economists—most notably Irving Fisher—who lost their shirts because of bad forecasts. But there are counter examples. One case was Richard Cantillon, one of the founders of economic science, who made two fortunes on the basis of his understanding of the internal inconsistencies of John Law's "economic system."

Cantillon's second fortune is most instructive. One of the aims of Law's system was to displace specie with bank notes and stock. To do this, Law, as finance minister, announced on March 11, 1720, a series of depreciations of the value of specie in terms of the money of account or, alternatively, appreciations in the value of bank notes, and he imposed extensive exchange controls. Simultaneously, by issuing bank notes, Law continued to support the price of Mississippi stock. According to Antoin Murphy (1987), Cantillon realized the inconsistency of an appreciating exchange rate and an expanding money supply, so he sold French bank notes and bought Dutch guilders using his international banking connections to evade the exchange controls. Events proved Cantillon right—a run on the Banque Royale's specie reserves forced Law to reverse his policy, precipitating a massive loss of confidence and a quick collapse of the system. Cantillon made a fortune and fled the country under threat of the Bastille.

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McCloskey's Insight

The fact that Cantillon used his economic analysis to make a highly profitable forecast does not mean that others could repeat his experience. The key insight of McCloskey's paper is the point based on Friedman and Lucas. You can spend billions of dollars building elaborate econometric models of the economy that may be very useful in aiding our understanding of the economy's structure. But when these models are used for forecasting and control purposes, the outcome can only be disastrous.

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Better Forecasting or Better Rules?

W. Lee Hoskins

For sometime I have argued that the Federal Reserve needs better rules because it cannot forecast well. I have also felt that better forecasts probably would not help much. The reason is simply that I do not share the same policy goals as those who spend so much time and effort in trying to forecast the real economy. The Fed cannot eliminate the business cycle; the best it can do is minimize the instability coming from monetary policy by stabilizing the price level. For several years, I have argued that the better rule would be a target path for a general price index. Although somewhat of a misnomer, this policy has become known in the press as "zero inflation."

Forecasts and Forecasting Accuracy

The Fed maintains an elaborate process for economic forecasting because it is asked to fine-tune the economy. Congress delegated this objective to the Fed through legislation that mandates a variety of economic goals including stable prices, stable exchange rates, and maximum production. When exchange rates shift or trade accounts become imbalanced, pressure is placed on the Fed to correct the situation through monetary policy. Similarly, when the economy slows, pressure is placed on the Fed to turn its attention toward economic growth. In effect, the central bank is often in the position of a person who, in attempting to serve all masters, is ultimately able to serve none. Relying on monetary policy to achieve multiple goals—many of which are beyond the Fed's reach—may cause us to make mistakes, possibly serious ones.

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Using monetary policy to fine-tune the economy is fraught with peril. The record suggests that near-term, real GNP projections are too inaccurate to be of much value in determining the appropriate course of monetary policy from quarter to quarter. A recent study at the Federal Reserve Bank of Cleveland found that quarterly forecasts up to one year ahead reduced uncertainty by roughly 14 percent for the growth rate of real GNP and by 52 percent for inflation. For example, the mean quarterly growth rate of the economy between 1968 and 1985 was 2.6 percent (at an annual rate). On average, that is just about what forecasters predicted. But policymakers usually concentrate on the near term, and here the forecasting record is poor: The average one-quarter-ahead forecast error between 1968 and 1985 was about 4.2 percent. If we compare this average error to the 2.6 percent average quarterly growth rate of the economy over this period, we see that the representative forecast is unable to distinguish between an economy headed for prosperity and one on the verge of recession.

I often argue that this sort of uncertainty precludes a fine-tuning policy. But perhaps there is a more important point to make. The problem is not forecasting accuracy; the problem is that the Fed has accepted a responsibility to try to achieve an objective that requires both a degree of forecast accuracy and a detailed knowledge of economic structure that do not exist.

Improving the Policy Process

Any attempt to stabilize a price index directly will also require some data gathering and forecasting, but the success of the policy will not require such a high degree of knowledge or forecasting accuracy as is needed to stabilize real GNP. The key to effective policy is not the accuracy of economic forecasts but rather the credibility and predictability of policy actions. The more credible the commitment to price stability, the more limited will be the market reaction to adverse events. The more predictable the policy reaction to unforeseen economic events, the fewer wrong decisions will be made.

The policy process today, with its focus on the near-term economic outlook, does not provide as clear or credible policy objectives as I would like. More specifically, policy lacks an explicit, attainable objective. Under the current policy process, the relative importance of the various objectives changes with economic fluctuations. To accurately assess past and future decisions, market participants must constantly update their best guess about the central bank's long-run

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objectives. In the current environment, the market monitors policy actions to detect policymakers' intentions. But the lack of a clearly defined, long-term goal causes market expectations of the goal to vary with the latest economic news. This uncertainty reflects a monetary policy that is neither predictable nor credible.

Economic decisionmakers require more information about the long-run goal of monetary policy; they also require more compelling reasons to be confident that the Fed will indeed achieve that goal. Such confidence requires a monumental change in the current policy process. House Joint Resolution 24, sponsored by Rep. Stephen L. Neal (D., N.C.), would help bring about this critical change in policy by mandating that the Fed make price stability its primary objective.

A Stable Monetary Yardstick

Experience has shown that there are no quick fixes in the promotion of economic growth. We have no evidence that a faster trend rate of output growth can be bought with a higher rate of inflation. In fact, inflation reduces the general welfare by creating inefficient decisionmaking and thus lowering wealth-enhancing productivity. Viewed in this light, a monetary policy that best encourages long-run economic growth is one that is designed to eliminate inflation and the uncertainty associated with inflationary policies.

In theory we could eliminate the uncertainty about inflation with a 4 percent inflation rule, but in practice it would not work. I prefer a target of zero inflation. Zero is special. It eliminates the economic costs of the interaction between inflation and the tax system, and it is more likely to be credible. The costs of achieving zero inflation would be quite low in a policy regime with precommitment, and there is a substantial social benefit to a stable monetary yardstick.

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Toward Forecast-Free Monetary Institutions

Leland B. Yeager

> The beginning of wisdom . . . is to know that the future is unknowable. . . . Recognizing the inscrutability of the future requires . . . humility and intellectual self-discipline. It requires the candid recognition that human history is a discontinuous process, rather than the neat projection of established trends. . . . But the occasional awareness of our limitations is quickly elbowed aside by our all too human eagerness to define, right now, the shape of things to come.
>
> —Irving Kristol[1]

> [O]ur future is not determined by mathematical curves but by our own intelligence and will. But if this is so, the whole so-called science of business-forecasting inevitably becomes very much discredited. What the economist can do is to examine present facts and proposed lines of action, and to show how they are likely to influence the development of economic life. But he can never make a prediction of our future independent of our own actions. And we should never lose sight of the fact that the future is influenced by coming events about which we know nothing, and the prediction of which in any case does not belong to economic science.
>
> —Gustav Cassel[2]

> If you must forecast, forecast often!
>
> —Anonymous[3]

The Passion for Forecasts

A passion for forecasts carries to an extreme the "passion for news" diagnosed, with amusing exaggeration, by Jacques Ellul (1967,

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[1]Irving Kristol, quoted from *Fortune*, February 1969, by Fiedler (1990, p. 130).

[2]Cassel ([1927–28] 1951, pp. 332–33).

[3]The second of "three central laws of forecasting" cited by Fiedler (1990, p. 141). Many years ago I heard or saw this maxim attributed to either Marcus Nadler or Jules Backman, professors at New York University.

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pp. 53–63). News entertains. Being *au courant* serves one's sense of prestige. Unconcerned with enduring principles and connections, losing any sense of continuity, the ordinary citizen excites himself only over the latest events. Reflection would involve the news of the day before yesterday rather than just of this morning. To avoid drowning in the flood of news, he must forget. "[T]he more superficial, unimportant, and spectacular the information, the more people will be interested in it. . . . public opinion revolves only around problems of the immediate present" (Ellul 1967, p. 55). Someone living in the news demands immediate solutions, perhaps sensing that tomorrow he will have forgotten the problem exciting him today.

Gordon Williams's brief radio broadcasts, supposedly on economics, illustrate these passions. They have nothing to do with economic principles—unless one so counts the notion that spending (other than on imports) is good and more is better. Williams is preoccupied with the latest officially released economic number and even with forecasters' guesses about a number scheduled for release that morning. His and other broadcasts often say not that the stock market closed yesterday at 2572.8 (or whatever) on the Dow but that the market will open this morning at that figure. Speaking that way seems more up-to-the-minute and future-oriented than reporting what is, after all, a numerical detail of recent history.

The passion for news and forecasts shows up in media discussions of whether inflation is dead and whether the Federal Reserve should turn its attention to "fighting" something else. This attitude gives policy a short-run bias. The typical commentator seems to lack understanding of or concern for unintended policy consequences working themselves out only over time in unforeseeable ways.

Foretelling the Future versus Scientific Prediction

Accurate economic forecasts (beyond short-run extrapolations, anyway) are hardly possible apart from forecasts of all human affairs. So-called economic behavior depends on innumerable factors, including noneconomic ones and including people's theory-conditioned and subjective reactions to their experiences. Almost by definition, history is the unfolding of unique events and combinations of events. Minor causes can have major consequences, as the recently fashionable mathematics of "chaos" should have impressed on economists. If only Queen Victoria had been a man—if only one microscopic detail had been different at her conception in August 1818—the crowns of Great Britain and Hanover would have

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remained linked, and subsequent history might well have unfolded quite differently from the tragic way it did.

Because history is unique, foretelling the future is fundamentally different from the if-this-then-that predictions of natural science. A chemist can predict the result of placing zinc in hydrochloric acid, but he cannot foretell how much zinc and how much acid will generate how much hydrogen in a particular year. Similarly, it is unreasonable to expect an economist to foretell a country's balance of payments or inflation rate or interest rates in the unique historical circumstances of a few years later. Astronomers can foretell events within our solar system because known bodies move subject to known forces, with outside disturbances essentially absent. An economic system, in contrast, responds to all sorts of changing outside influences.

Econometric research can take advantage of the unplanned experiments cast up by history to shed some light on whatever dependable relations may hold among some economic magnitudes. Research of this sort is not the same thing as foretelling the future and hardly justifies adopting policies that presuppose it.

Degrees of Dependence on Forecasting

But even if forecasts are unreliable, what alternative do we have to making them? Must not decisions of all sorts rest on judgments about the future? Isn't budgeting indispensable, even though largely an exercise in forecasting? Well, yes, but a distinction holds. Forecasts are more crucial to some arrangements and policies than to others. The mistake is to depend on them needlessly. It is sensible to avoid, when we can, making ourselves dependent on trying to do what we cannot do well.

Forecast-dependent policies require foretelling prices, output growth or recession, unemployment, interest rates, balances of payments, or whatever, and then, if these outcomes are judged unsatisfactory, trying to make them turn out differently. In making so much depend on the subjective judgments of the authorities, such policies make the economic environment less predictable; for they set private decisionmakers to guessing what the authorities will do. Financial journalists plausibly relate many episodes of volatility in the stock, bond, and foreign-exchange markets to uncertainties and changing conjectures about monetary policy. Injecting avoidable uncertainties about policy tends to waste the scarce human capacity to cope with the change and uncertainty that is inescapable.

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A contrasting type of policy relies, instead, on something more akin to scientific if-this-then-that prediction: It involves examining the likely operating properties of alternative sets of institutions and choosing the set judged to have the most attractive properties on the whole. It holds down the scope of frequent large centralized decisions, whose effects are harder to cope with than the gradually occurring cumulative effects of innumerable decentralized private decisions.[4]

All planning necessarily looks to the future. But just as the logic of a market economy recommends *decentralized* planning, so it recommends "competition in prediction" as "an integral part of competition in the wider sense. . . . [T]he many different views of the future held by independent operators cannot . . . be aggregated into a 'common view.' . . . [D]ecentralization of the forecasting function [is] one of the advantages of the market economy over the centrally-directed economy. . . . [It is] natural and desirable that the economy should work to a plurality of views, rather than to a single view of [the] future" (Lutz 1969, pp. 149–50, quoted in Nutter 1983, p. 118).

Trying to impose conformity on the market's multitude of forecasts risks compounding errors. If, for instance, a central authority substitutes its own single five-year forecast of oil supply and demand for the variety of forecasts that individual decisionmakers would otherwise derive from their own observations and foresight, unnecessary wastes will occur. The very spread in mistakes distributed among independent forecasts, involving overlapping margins of error, would bring differential adjustments in expectations and behavior that would diminish average forecasting error over time (Nutter 1983, pp. 118–19).

The Example of a Price-Level Rule

Targeting on a price index exemplifies a relatively forecast-free and discretion-free policy (although other reforms might excel it in these respects). An unambiguous rule relieves the monetary authority of constantly reconsidering what weights to give to fighting inflation, resisting recession, promoting employment, stimulating growth, improving the balance of payments, making credit easy, aiding government finance, appeasing politicians, and pursuing other desired results. One clear objective is less difficult to attain when its possible rivals are out of the way, and the authority's performance becomes easier to monitor. Private expectations can

[4]In not-yet-published papers, Roger Koppl sets forth the "big-players argument" concerning the disruptiveness of large-scale decisions and market interventions.

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crystallize around price stability, which further facilitates the authority's task.

Criticisms, like the proposal itself, are old and familiar. Imbalance between money's supply and demand shows up in the target price index only with a lag. Further lags between index movements and policy responses and their impact might make those responses perverse and destabilizing when they took belated effect. But this difficulty would presumably plague a policy of sharp shifts, not a steady policy. Steadiness is easier in pursuing a single goal than multiple goals with changing weights. Like a good driver, the authority might make frequent small corrections instead of belated sharp swerves. Through continual diagnosis of price-level pressures—distinct from ambitious forecasting—the authority would try to avoid blundering off course. The authority might stay alert to incipient inflation or deflation signaled by industrial-production figures, exchange rates, interest rates and their term structure, and sensitive commodity prices determined in auction markets (Johnson 1988). These indicators should remain just that and not become rivals of the price-level target.

Watching sensitive commodity prices does not presuppose that they move dependably in parallel with the consumer price index; they do not. In the long run the two sets of prices drift apart under real as opposed to monetary influences. In the short run commodity prices are more volatile and respond more quickly to monetary disturbances (Boughton 1989, Marquis and Cunningham 1990). The latter contrast recommends commodity prices as a tool for diagnosing disequilibria that, left uncorrected, would in time inflate or deflate consumer prices and, in the deflationary case, would also temporarily shrink real activity.

A modified version of Irving Fisher's (1920) compensated dollar would further limit any authority's discretion, circumvent the problem of lags, and lessen the need for forecasts or even for continuous diagnosis. The authority would be required to maintain two-way convertibility between its money and whatever changeable amount of some redemption medium was actually worth, at current prices, the bundle of goods and services specifying the target price index. (More exactly, the bundle would *define* the dollar.) If the dollar always exchanges against just enough redemption medium (possibly gold, but probably securities) to be worth the bundle, then the dollar is worth the bundle itself. The authority's obligation to redeem its money in this way at the holders' initiative puts teeth into its commitment to a dollar of stable purchasing power. Private arbitrageurs and speculators, understanding the system, would reinforce this stability.

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This solution to the question of *how* to implement a stable-price-level rule admittedly seems too simple to be genuine; but if so, I await seeing its flaw identified.[5]

Objections to Stabilization

Objections to stable money mentioned so far are really objections to more or less tacitly assumed *methods of implementing* the policy. Some modern "Austrian" economists, in particular, worry about "injection effects" of expanding the money supply even merely to keep the price level from sagging in a technologically advancing economy. Their well-known theory of the business cycle focuses on the consequences of falsifying interest-rate signals through monetary expansion. George Selgin (1990, pp. 277–81) stresses temporary widenings and subsequent painful narrowings of profit margins associated with delays in factor-price responses to spurts of productivity improvement under such a policy.

Arguments for price-level stabilization sometimes tacitly assume that gains in productivity come unanticipated. This assumption is usually inappropriate, Selgin suggests, as a basis for worry about how prices respond. Price-setters in directly affected markets will be alert to productivity improvements. Many will even have initiated them and will promptly pass cost cuts into prices. No pains demand avoidance through stabilizing the price level when productivity rises. Yet such a policy would expand money incomes, swelling profits temporarily unless it were perfectly understood and anticipated and promptly reflected in factor prices.

If productivity *falls*, monetary contraction to resist a rise in the price level shrinks nominal income and depresses profits. It discourages producers by making them bear "more than their fair share of the overall burden of reduced production"—until workers and other sellers of inputs belatedly accept painful cuts in wages and other factor prices (Selgin 1990, p. 279).

Avoiding abnormally high or low profits or profit expectations is more crucial to maintaining monetary equilibrium, Selgin insists, than price-level stability. Only under what he recommends as the "productivity norm," whereby the price level varies inversely with overall factor productivity, does aggregate demand remain adequate

[5]Admittedly one might imagine a "paradox of indirect convertibility" plaguing a system in which money is redeemable not directly in the good or goods defining the dollar but only indirectly in some convenient medium of equivalent value. Knut Wicksell expressed such a worry in 1919. W. W. Woolsey and I believe that we have refuted misconceptions on this topic, along with clarifying genuine difficulties, in our article of 1992.

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but not excessive for buying full-employment output at prices covering money costs of production. The productivity norm also avoids distorting interest rates away from their natural levels (Selgin 1990, pp. 280–81).

Ways of avoiding price deflation without monetary injections are mentioned toward the end of this paper—in case injection effects really are worrisome. Rather than repeat what I have said elsewhere on this issue, however, I turn to more fundamental issues.

Productivity, Equity, and the Price Level

Many economists have denied that a stable-valued money unit is desirable, even apart from the difficulties of achieving one. They go beyond acknowledging complaints about how unexpected price inflation or deflation redistributes wealth between creditors and debtors. Even the distributional effects of *stability,* especially in the face of changes in productivity, draw criticism. David Davidson (1906 and other articles listed in the references) invented hypothetical examples. Stable prices would keep a creditor from sharing in the gains from a general rise in productivity, while someone who had borrowed for productive purposes would unfairly keep the entire gain for himself. A rise in the productivity of land would tend to depress the prices of its products and so not unambiguously either raise or lower the value of the land itself. A monetary policy of stabilizing the product price level, however, would raise land's money value. A landowner who had leveraged his holding by debt would gain relative to a debt-free owner, which seemed unfair to Davidson.

Selgin (1990, pp. 273–75), resurrecting related arguments, contends that when the price level falls because of generally improved productivity, debtors do not suffer, since their real incomes rise along with the real value of their debts. All they miss is an opportunity to enjoy an undeserved windfall at creditors' expense. In the opposite case of an adverse supply shock, preventing a rise in the price level would require an unfair *contraction* of all nonfixed money incomes.

Such arguments about the distributional unfairness of stable prices seem weak in the context of a long-term productivity uptrend. Investors, lenders, business firms, and other borrowers will allow for expected productivity gains in interest rates, in equity participations in loans, in issue prices and other features of corporate stocks, and in innumerable other terms of their financial transactions.

Worry about unfairness from adverse supply shocks seems more plausible. If, however, the monetary system is credibly committed

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to price-level stability even despite shocks, people will allow for their possibility in making contracts, including the mix of loan and equity elements in financial transactions. On this particular score, long-term loans will bear lower interest rates than they would in the absence of the price-level guarantee. In effect, long-term lenders pay an insurance premium for shock protection by accepting lower interest rates than if they bore the risk themselves. If an adverse shock does occur and creditors gain from a price level nevertheless kept stable, then they are in a position like that of a householder who "benefits" from having been insured (and having paid the premiums) when his house burns down. People and firms owing debts fixed in a stable unit of account do lose from an adverse supply shock, but they had presumably seen an advantage in borrowing at a lower rate of interest than they would have had to pay on loans denominated in a depreciation-prone unit; they are in the position of insurance companies.

To change the analogy, people who gain from holding claims denominated in a stable unit are in a position like that of stockpilers of oil who reap a "windfall profit" if an energy crunch occurs. In either case, do economists really recommend redistributing the gains and losses resulting from good and bad foresight and luck? (These are gains and losses judged relative to the distribution that would have emerged from a different course of events.) Do economists really recommend operating a monetary system to second-guess the parties to voluntary contracts?

A known and credible price-level policy at least provides a framework within which contracting parties can allow for contingencies as their own diverse circumstances, knowledge, and attitudes toward risk suggest. Can we really expect better results from centralized administration of foresight, risk-bearing, and their distributional consequences? The literature on rational expectations further suggests why the distributional case against stable money is a red herring.

Real and Monetary Influences on the Price Level

More narrowly economic considerations require closer attention. Critics of price-level stabilization sometimes agree about avoiding money-side disturbances but want to accord influences on the goods side their full natural scope. If rising productivity expands the supply of goods, a decline in prices is the natural response. Yet this distinction bears little weight. Growth in income and in the quantities of goods to be traded operates as much on the money side, expanding the demand for real cash balances, as it operates on the goods side.

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I wonder whether the idea that money prices should reflect the "real" cheapening of goods in general does not rest on some inchoate illusion that money has a value of its own distinct from what it will buy. Earlier (Yeager 1988, pp. 271–72), referring to David Davidson (1906) and Benjamin Anderson ([1917] 1922), I said that these economists had tried "to distinguish, though not in a way intelligible to me, between the value of money and its purchasing power, the reciprocal of the price level."

After further study of Davidson's writings (listed in the references), I now think I see what he meant. Gustav Cassel had forthrightly identified changes in the general price level with changes in money's value. A general rise in prices, Davidson objects, can reflect either a rise in the value of commodities or a fall in the value of money—or a rise in the value of both, with commodities gaining value in greater proportion, or a fall in the value of both, with commodities losing value in lesser proportion. Davidson (1923, p. 197) even presented a table purporting to show how much of the rise of prices in Sweden during World War I traced to an increased scarcity-value of commodities and how much to a decreased scarcity-value of money.

He accepted a real-cost theory of value and was even trying to improve Ricardo's mainly labor-input theory.[6] If increased productivity reduces quantities of labor and other primary factors necessary for a unit of output, then goods have really become cheaper, in Davidson's view; and their prices, expressed in money of stable value, go down.

Without going into detail, Davidson hints at how to reconcile this real-cost doctrine, more or less, with a marginal-utility theory of value. If goods become more abundant than before, then, precisely in accordance with the principle of diminishing marginal utility, their marginal utility and value decline. If effects like those of a decline in productivity occur, as when Sweden's international terms of trade worsened during World War I, then goods have higher marginal utility and greater scarcity value than before.

For money, too, lesser or greater scarcity (relative to population, as Davidson occasionally says) entails lesser or greater marginal utility and value. Davidson warned against losing sight of how the values of goods and money might separately be changing. Concern only with their ratios of value would be like concern only with how the ratio of the average heights of women and men had changed

[6]His article of 1919 addresses the theory of value in general, without special reference to money. It is a pity, says Thomas (1935, p. 47), that Davidson spent fruitless effort on revising classical value theory.

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over some period, neglecting what had happened to the average absolute heights of women and of men (Davidson 1909, p. 12). To advocate money not of stable value of its own but of stable purchasing power as measured by some price index is as "metrologically absurd" as wanting to adjust the definition of the meter according to changes in the average absolute length of objects measured; it is like wanting a separate meter for children, shorter than the adult meter (Davidson 1922, p. 113).

Yet is it not true that all measurement is necessarily relative? There are no utterly absolute standards—are there?—of length or mass or value or anything else. Rising productivity cheapens some goods relative to others (notably, consumer goods relative to human effort), but it can hardly cheapen goods and services in general relative to goods and services in general. Each good's price expresses its value relative to others when prices are quoted in a unit of stable general purchasing power.

Letting the price level reflect changes in productivity seems more plausible when specific goods, not general trends, are in question. Suppose that technological progress cheapens some particular good and so reduces the average price level slightly as a matter of mere arithmetic (Selgin 1990, esp. p. 275). This decline evidences no excess demand for money undergoing perhaps sluggish correction. By hypothesis, producers have cut the affected good's price promptly and painlessly in line with its reduced cost. Its output presumably increases, perhaps along with outputs of other goods into whose production factors may have been released. The real volume of transactions to be lubricated increases and so does the associated demand for real cash balances. Money's rise in purchasing power automatically accommodates that increased demand (but accommodates it only more or less, for only by extreme coincidence would the pattern of interrelated price and quantity adjustments and of income elasticities of demand for real balances make the accommodation exact).

If only one particular good were ever to become cheaper through technical progress, that fact would argue against choosing it to define the money unit. (If only gold production kept gaining in technical efficiency, a gold standard would be inexpedient.) We would even want to omit that exceptional good from any bundle of commodities defining the unit or used to calculate a target price index. Rather than inflate other prices to stabilize the average, it would be simpler to let the price of the exceptional good fall.

More generally, whenever technical progress affects one good only, we might like its price to fall without disturbing any others. No unit is available, however, in which prices could behave that

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way. Substitutabilities and complementarities in consumption and production and other aspects of general interdependence keep any single price from changing *alone*. It is pointless to wish for a unit with impossible properties (like one whose adoption, besides offering all plausible benefits, would also prevent drug addiction and sloth among secretaries).

It is misleading, furthermore, to consider goods affected by technical progress only separately, one by one. Pervasive contributions to productivity, including capital accumulation and gains in knowledge, affect broad ranges of goods over long time spans. Goods cannot all fall in price relative to each other. The operational question becomes not "Why inflate other prices when a single price falls?" but rather "Why not absorb what would otherwise be a general downward pressure on prices?" Why express money prices in a way that requires most of them to fall even though relative prices are changing in diverse ways? It seems counterintuitive to suppose that individual price changes would be fewer when they were negative on average rather than zero.

Selgin (1990, pp. 275–76), though not sharing my intuition on this issue, recognizes that no rigorous argument is available to settle it. How productivity gains may affect prices is far from straightforward, by the way, as Wicksell noted in 1909. Inventions or other developments promising to raise productivity may stimulate investment spending and so initially tend to raise prices. The question of the time pattern of productivity effects thus poses additional complexity for any notion of optimal responsiveness of the price level, as distinct from stability.

Productivity, Factor Prices, and Income Targeting

Considerations like Selgin's, perhaps along with Davidson-like notions of objective value, suggest defining the money unit by a bundle not of products but of labor and other primary factors of production. Davidson (1922) did have that idea, but practical difficulties recommended a rough equivalent to him—money managed to stabilize average nominal income per member of the population. David Glasner (1989, chap. 11) advocates money stabilized against an index of labor wage rates. While ideally preferring stabilization of money income per worker, Selgin (1990, p. 272) recognizes stabilizing per capita income as a practical approximation. That policy would come close to his productivity norm, making an adequately flexible price level vary in roughly inverse proportion to average productivity.

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As Selgin (1990, p. 282) recognizes, his proposal loosely resembles currently popular ones for targeting monetary policy on nominal income. These proposals do not envisage fixing income per person, however. Instead, total nominal income or gross national product (GNP) would trend upward at a rate thought consistent with average price stability over the long run. Bennett McCallum (1987 and 1989, chap. 16) explains a rule aiming at this result. Michael Bradley and Dennis Jansen (1989) describe nominal-GNP targeting as a straddle between price-level and real-output targeting, the latter being quite inappropriate for reasons one hopes are familiar. Nominal targeting would tend to stabilize "real GNP at its natural rate of output," and "automatically, without monetary policymakers having to know what the natural rate of output actually is" (Bradley and Jansen 1989, p. 40). James Hoehn (1989) claims further advantages for that policy.

The advantages of targeting on nominal GNP arguably extend to supply shocks. In McCallum's version, an automatic-feedback rule avoids reliance on episodic forecasting. Like any reform, however, such a rule implies a prediction of its operating properties, which implies a forecast about the economic environment and its interaction with features of the proposed institution.[7]

Several points seem to count against nominal-GNP targeting. First, the target is conventional, constructed, less continuously available, more subject to delays in reporting, more open to revision, and less directly observable by the ordinary citizen than prices. (The underground economy contributes to inaccuracy.) A price index or the total price of a specified commodity bundle has its conventional and constructed aspects, too, though in lesser degree; the ordinary citizen has a more nearly direct view of prices than of GNP. Second, by its nature, the GNP target must be pursued by a central monetary authority, which must be concerned with financial innovations that might loosen its control over its target. Alternative reforms could give freer rein to financial innovation. Third, centralized GNP targeting lacks the discipline of competition that would operate under private-enterprise-oriented reforms. It is easier for a central monetary authority to miss its targets by a little and eventually by a lot without coming under direct corrective pressure.

Adversity

Perhaps the most embarrassing case for advocates of stable money is a sharp drop in productivity or the equivalent—a supply shock

[7]Compare Viner (1962) on the long-run forecasting unavoidable in formulating any rule.

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worse than the oil shock of 1973–74, a war, or some other calamity. Such a shock is vividly imaginable in a small, economically specialized country depending on imports paid for by one or a few export products. The country is vulnerable to worsening of its terms of trade or, say, to failure of a major export crop. If a severe loss of income and wealth must be quickly allocated over its population somehow or other, an inflationary tax on cash balances and nominal incomes can hardly be ruled out a priori as a one method.[8]

Suppose, furthermore, that the shock directly raises some specific prices and others closely linked with them. The pattern of relative prices suffers initial distortion, which obstructs market-clearing. Mechanically, arithmetically, the average price level rises. Total real money balances shrink, and with them the volumes of transactions, production, and employment they can support. Trying, nevertheless, to hold the average price level steady by tightening the money supply to restrain the rise of the most directly affected prices and to strengthen downward pressure on other prices would worsen this recessionary shrinkage of real money balances.

An opposite policy might seem more sensible—resisting unemployment by partially restoring real balances through monetary accommodation of the inflationary shock. In the long run, it is true, such monetary accommodation would be unnecessary. Market pressures would in time overcome price and wage stickiness and would achieve declines in other prices averaging out the upward shocks to particular prices; maintaining the steady target price level would prove compatible with market-clearing. In the meanwhile, however, the economy would have suffered exceptional unemployment. Perhaps it would be reasonable, after all, to try to mitigate this consequence by tolerating and even monetarily ratifying the shock-imposed initial "arithmetical" rise of the average price level, at least temporarily.[9]

I see no logical or factual error in such a case. I even think it provides the strongest argument available against the goal of stable money. A valid argument is not necessarily decisive, however; other arguments may well pull the other way. An abandonable goal of price-level stability would less fully enlist the support of private expectations than a firm goal would. How sticky the "other" prices (and wages) are that would have to decline to average out the shock-

[8]Such cases apparently persuaded Wicksell, toward the end of his life, to qualify his call for a stable price level (Uhr 1962, pp. 300–305).

[9]This case for allowing temporary departures from a price-level target is a main theme of Hall (1986).

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imposed rise of specific prices is surely not independent of the policy rule and related expectations. A policy of accommodating price-raising shocks would increase people's reluctance to cut these other prices and wages that would otherwise come under downward pressure. (Why cut a price or wage if monetary expansion is likely to make the cut unnecessary?) A policy of accepting and supporting a shock-induced rise in the price level would thus worsen the very stickiness that seems to recommend that policy. A firm and credible commitment to a stable price level, on the other hand, would encourage price-setters and wage negotiators to yield to market pressures for market-clearing adjustments; and these responses would hold down the unemployment costs of price-level stability.

Worry about shocks applies less to a large, diversified country than to a small one, especially if a long-term productivity uptrend affords scope for absorbing moderate shocks through mere temporary slowdowns or interruptions in the growth of nominal incomes at a steady price level. A large, growing economy enjoys insurance, so to speak, from the law of large numbers: While at any time a few of its sectors may be suffering adversity, many other sectors are likely to be prospering.[10]

No such considerations amount to claiming that any country's institutions can be made invulnerable to calamities. They cannot. Institutions should be chosen to serve and improve the relatively normal conditions in which they have a good chance of flourishing. Shaping institutions for the worse conceivable cases instead is perverse and reminiscent of the maximin criterion for income distribution recommended by John Rawls (1971).

One might even argue that stable money provides a better starting point for government borrowing and money issue in rare emergencies than money commanding little confidence in the first place. Such an argument was made in the late 19th century for putting Russia's floating paper currency onto the gold standard.

Choice among Institutions

One general point demands emphasis. Ingenuity can produce innumerable particular cases in which price-level stability—like any other monetary rule or regime—brings results deemed inferior, on the specific grounds considered, to some alternative rule or regime tailored to the specific circumstances of a particular economic sector at a particular time. Yet monetary regimes can hardly be installed

[10]Some considerations cited in the theory of optimum currency areas evidently also apply to choosing monetary institutions for an area taken as given.

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and altered to serve specific cases. Fundamentally, economic policy means choosing and modifying *institutions*—the rules and constraints within which individuals, families, firms, and government agencies act (Vining 1984). Policymakers have no direct handle on outcomes—prices, allocation of resources among different lines of production, geographic distribution of productive activities, patterns of employment and unemployment, and distributions of income and wealth.

For monetary regimes, the basic institutional choice concerns the unit of account—the unit in which prices are set, accounting conducted, costs and benefits estimated, and contracts drawn. Is the unit to be some particular commodity or composite of commodities, perhaps chosen for the expected behavior of its value relative to goods and services in general? Or is the unit to be some fiat currency whose value depends on its scarcity relative to the demand to hold it, a scarcity regulated by a monetary authority?

Adopting a fiat currency as unit of account implies choosing some principles for its management, but that adoption still cannot achieve some detailed pattern of economic outcomes. Of course, one may join Davidson in thinking up particular constellations of circumstances and propounding ethical judgments according to which fairness between debtors and creditors or among other groups might better be served by a fall (or rise) of the price level than by its stability. If, however, the balance of considerations favors institutions achieving monetary stability over alternative institutions, then it is simply irrelevant to think up particular cases in which some other price-level behavior might seem preferable. Institutions and rules cannot be switched on and off from case to case. It is unreasonable to expect a monetary system to achieve all sorts of good results, including economic justice as each person understands it, in the face of multifarious changes in conditions. Theories of rational expectations cast further doubt on the idea that the choice of monetary regime can reliably influence real economic outcomes, such as the distribution of real income and wealth.

No single set of institutions has advantages only, free of any disadvantages. Tradeoffs are unavoidable in institutional choice. Unwillingness to face them is paradoxical among economists, whose subject's most basic fact is the impossibility of having all good things at once.

A monetary system should do what it can reasonably be expected to do, leaving other institutions to undertake tasks more suitable for them. A stable unit of account at least facilitates economic calculation, planning, and contracting. As for fairness, savers need not

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restrict themselves to buying interest-bearing securities of fixed nominal value. They can try to take account of prospective changes in productivity in various industries by investing in equities. They can diversify their asset portfolios, directly or through mutual funds. Their portfolio choices can express their different degrees of willingness to bear risk. Business firms can raise funds not only by borrowing in nominal terms but also by obtaining loans with equity participations or by selling stock. A sound monetary system improves such opportunities by facilitating financial intermediation and innovation.

As a gesture toward completeness, we should briefly note some further leading arguments for and against a stable unit of account. No one argument, by itself, is decisive, and some arguments are disputed. First, inflation adds "noise" to nominal prices, it degrades the information they contain, and inaccuracies in price comparisons cause allocational inefficiencies (Gavin and Stockman 1988). Unpredictable, "ragged" inflation, especially, undermines economic calculation and long-run planning. The savings-and-loan mess provides an example. One aspect is that S&Ls found themselves locked into long-term assets at the old nominal interest rates of times before inflation speeded up. More generally, unexpected inflation and accelerations and decelerations redistribute wealth capriciously. Second, inflation interacting with the tax system and its depreciation rules distorts production and investment. Third, efforts to avoid losses of purchasing power on money and other dollar-denominated assets require spending real resources to keep down the size of these holdings. These efforts also breed new financial institutions and instruments that would otherwise be inefficient and unprofitable. Politics interacts with financial innovation, regulation, and deregulation in determining the details of the changes made. Fourth, by targeting on the price level and quickly moving to reverse any changes in it, policymakers would gain credibility and reduce uncertainty (Gavin and Stockman 1988; compare Bryan 1990).

Among other arguments against stable money besides those invoking adverse supply shocks, probably the one most commonly met nowadays does not actually condemn price stability; instead, it stresses the pains of squeezing a long-entrenched inflation out of the economy.[11] The desirability of a goal and possible difficulties of

[11]Rao Aiyagari (1990) exemplifies an excessively narrow view of the costs of inflation. Briefly, he classifies them merely as the "shoe-leather" costs of keeping cash balances smaller than otherwise, overtaxation through underdepreciation, and confusion between changes in the price level and in relative prices. He overlooks the importance of a dependable unit of account for economic calculation and long-term planning and contracting. He does not identify, for example, the ways in which current disasters in the financial sector trace to past absence of such a unit. He overlooks various costs of

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attaining it are, however, distinct topics. Furthermore, inflation as we know it is bound to occur at fluctuating rates: Relative disinflation from time to time, along with its actual or supposed pains, is unavoidable anyway. Steady inflation is a "mirage" (Okun 1971 and 1981, pp. 283–84). Relatedly, a policy of keeping inflation steady at a positive rate is not credible and so can hardly serve as a focal point of private expectations, whereas there *is* something special about a rate of *zero*.

The Unit of Account and Free Banking

Rejecting price-level stability means rejecting a unit of account defined by a bundle of goods (and services), whether defined directly or defined indirectly through targeting on a price index.[12] What unit of account, then, does the critic recommend instead? The case is weak for a unit defined by gold or any other single commodity.

Anyone recommending some sort of productivity or money-growth or nominal-income rule—or, at the extreme, recommending the discretionary monetary actions deemed best case by case and day by day—must envisage application of the rule or exercise of discretion by a central authority equipped with the necessary powers. This means—unless I am committing some gross oversight—that the unit is nothing more definite than a unit of government fiat money man-

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trying to cope with inflation, including the diversion of resources into seeking and providing supposed inflation hedges.

[12]An exception is barely conceivable. At the present stage of discussion, however, possibilities of bypassing some difficulties of a stable price level while giving the unit of account a commodity-bundle definition seem worth only a footnote. The bundle of goods and services defining the unit or the target price index might be specified with a variable composition in the first place. "Outliers" might be removed automatically: Those particular goods bearing a specific aggregate weight in the bundle whose prices had risen most (and possibly, also, those whose prices had fallen most) over the previous x months might drop out of the bundle. While the price level of this variable bundle would thus remain steady, the broader price level would be allowed to rise to accommodate adverse supply shocks (and perhaps, also, to fall when supply developments were favorable).

Just conceivably, one might define the dollar as a certain (very small) fraction of nominal GNP, with the amount of redemption medium into which a one-dollar note or deposit is indirectly convertible being suitably adjusted. Since nominal GNP depends on conventions of definition and measurement and is not directly and continuously observable and since such an approach requires a specific though growing total quantity of money, it is unclear whether the approach could be made compatible with competitive private issue of money and spontaneous adjustment of its total quantity. Credibility would be hard to achieve, furthermore, for a flexibly or complexly defined unit. The idea of an adjustable unit or price-level target opens a can of worms, some of them political. Still, some such ideas (largely due to W. W. Woolsey) may be worth further thought.

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aged, one hopes, in some satisfactory way. That choice of unit leaves the monetary system vulnerable to the government abuses to which the historical record testifies. It precludes a nongovernmental monetary system.

Choice of a commodity-defined unit of account, on the other hand—preferably one defined by a comprehensive bundle of goods and services—makes possible free banking as envisaged in several current proposals. Rather than again describe a proposal by Robert L. Greenfield and me, I'll simply liken it to Irving Fisher's compensated dollar of 1920 modified as mentioned earlier in this paper and further modified by placing the issue and redemption of money on a decentralized, private, and competitive basis.

Under such a system, the unit of account has its value determined quite otherwise than by supply of and demand for money, whether base money or media of exchange more broadly conceived. No authority ever has to "inject" money into circulation (or sometimes withdraw it) to make its supply match the demand for it at a level compatible with a price-level target or any other principle of monetary management. The supply of money (however exactly money might be defined), as one aspect of the supply of financial-intermediation services, accommodates itself to the demand for it at the stable price level corresponding to the definition of the unit of account. The supposed problem of "injection effects" mentioned earlier is simply bypassed.

Bypassed also is any need for central forecasting. Any forecasting tasks that remain are dispersed among competing private money issuers and speculators. Speculation, along with the indirect convertibility of money and the operations of clearinghouses and arbitrageurs, keeps the commodity-bundle definition of the unit of account operational. "Macroeconomic entrepreneurs," as one might call them, will gather information about current or foreseeable aggregate demand and supply shocks and use it in their transactions in securities and other assets. Their activities will help determine market interest rates and a quantity of money consistent with the independent definition of the unit of account.[13]

Conclusion

"How would you define the unit of account?" Persons who reject a unit of stable purchasing power and dream up cases in which

[13]Woolsey and I spell out this admittedly cryptic claim in our 1992 manuscript. Here I merely insist that government fiat money precludes this sort of decentralization and competition.

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change in the price level would be preferable should have their noses rubbed in that question. What definition of the unit of account would make the price level behave as they deem optimal? Usually, I conjecture, those persons are at least tacitly envisioning the unit of a fiat medium of exchange ideally managed by a governmental authority. Such people would solve monetary problems by assigning them to a philosopher-king.

Ideally managed government fiat money is beguiling. Each person can imagine its being managed as he deems best for each imagined set of circumstances. Apart from this chimerical aspect, the sorry history of government fiat money, now reinforced by public-choice theory, makes it doubtful that sound management would endure.

Fiat money managed to satisfy some macroeconomic criterion—its total or per capita quantity, total or per capita nominal income, a productivity norm, or whatever—precludes decentralizing and privatizing the issue of money. Free banking, however, could operate on the basis of a stable unit of account defined independently of any particular medium of exchange and instead defined by a comprehensive bundle of goods and services. The pressures of competition—competition from which a government monetary authority is exempt—would impose discipline on private money issuers, forcing them to keep meaningful the denomination of their bank notes and deposits (and checks) in the stable, independently defined unit.

Besides having other advantages, such a system would radically reduce the need for forecasting (whereas ambitious forecasting is necessary for ideal management of government fiat money, though less necessary for management bound to a price-level rule than management accorded greater discretion). Any forecasting functions that did remain would be healthily decentralized under free banking. Privatization of money seems to me, then, an attractive route toward forecast-free monetary institutions.

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On Foot-Loose Prices and Forecast-Free Monetary Regimes

George A. Selgin

The Choice of Monetary Regimes

Leland Yeager says that predictions about human affairs can seldom be accurate. True as this may be in general, it did not stop me from anticipating parts of his paper. I expected new arguments in defense of his favorite scheme for price-level stabilization, and that is what I got.[1] Nor do I believe that my forecasting ability is unique; Yeager is unlikely to be surprised by much of what I intend to say in reply. Perhaps one or both of us is guilty of having rigid beliefs, or at least of having beliefs that adjust "sluggishly."

Unfortunately for the conduct of monetary policy, the folks at the Federal Reserve cannot rely on any similar rigidity in the social world. For this reason, I fully concur with Yeager's suggestion that our present forecast-dependent monetary regime should be replaced by some forecast-free alternative. The question is "What alternative?" Yeager champions a particular stable price-level ($\overline{P}$) regime; I, in contrast, have argued for a "productivity-norm" or stable money-income ($\overline{MV}$) regime. Much of Yeager's paper is devoted to showing that his favorite regime is better than my favorite regime.

Actually, I may be the only living proponent of an $\overline{MV}$ regime. Sweden's David Davidson, the only other $\overline{MV}$-regime advocate mentioned by Yeager, died long ago. It seems, therefore, that Yeager has employed a sledge hammer to swat a fly. But has the fly been swatted? I say it has not. For although Yeager succeeds here and there in showing that an $\overline{MV}$ regime is not under all circumstances clearly superior to a $\overline{P}$ regime, he fails to provide a single instance in which

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[1]Admittedly, I had some inside information in the form of an earlier manuscript by Yeager containing some of the same arguments as the paper published here.

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it is definitely worse. Since Yeager admits that a $\overline{P}$ regime is inferior to an $\overline{MV}$ regime in at least one circumstance, the $\overline{MV}$ regime must actually be the better of the two all around.

A Price-Level Regime versus a Stable Money-Income Regime

Consider how the score unfolds swat by swat. Early in his paper, Yeager refers to my argument that an $\overline{MV}$ regime stabilizes aggregate demand while avoiding abnormal profits or profit expectations in the face of productivity changes. Yeager offers no rebuttal, but says he will suggest "ways of avoiding price deflation without monetary injections" and associated profit effects at the end of the paper. In fact, he never does show how prices can be kept stable in the face of improvements in productivity without some boost to nominal spending and (short-run) profits. If output prices are to remain constant as productivity improves, input prices have to rise. This rise will, assuming constant velocity, require an increase in the money stock. Score: fly 1, hammer 0.

Distribution Effects

On the matter of equity or distribution effects, Yeager wonders whether economists ought "to second-guess the parties to voluntary contracts." He forgets that it was $\overline{P}$ people (not including Yeager himself, though) who first diverted the hunt for sound policy with this ethical "red herring." $\overline{MV}$ advocates, starting with Davidson, were merely responding to $\overline{P}$ advocates in pointing out that equity considerations provide no stronger argument for $\overline{P}$ than they do for $\overline{MV}$. They also argued, "plausibly" in Yeager's opinion, that adverse supply shocks would be painful (if not "unfair") under a $\overline{P}$ regime, because such a regime would require a contraction of money incomes matching the contraction of real income to keep prices from rising. Yeager's reply, that borrowers could "allow for the possibility" of shocks by insisting on lower interest rates as a kind of shock "insurance," ignores the fact that such "insurance" must normally entail a lower level of lending, which some would be inclined to view as costly to society. In that case, the insurance argument amounts to a tacit admission that a $\overline{P}$ regime would be inferior to an $\overline{MV}$ regime, since the latter would make shock insurance unnecessary. However, because all of this is alleged to be beyond the purview of value-free economics, we will call the outcome a draw. The score is still fly 1, hammer 0.

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Money-Price Adjustments

Although Yeager admits the "plausibility" of arguments for $\overline{MV}$ instead of $\overline{P}$ when changes in productivity involve "specific goods," he finds the same arguments unconvincing when "general trends" in productivity arise. In that case, he argues, "Goods cannot all fall in price relative to each other." Therefore, he asks, "Why not absorb what would otherwise be a downward general pressure on prices" by expanding the money stock? The answer is that, when productivity is generally improving, the prices of goods must fall relative to the prices of factors of production (e.g., wage rates). Then, if downward movements in goods prices are to be "absorbed" by additions to the money stock, factor prices will have to rise: When productivity is changing, to stabilize output prices is to destabilize input prices. So it is by no means evident that a $\overline{P}$ regime would require fewer overall money-price adjustments than an $\overline{MV}$ regime, even in the extreme case where technological progress is all pervasive. Score unchanged.

I doubt whether citizens really have, as Yeager states, "a more nearly direct view of prices than of GNP," if "prices" refers to prices of goods in general (stability of which is the real object of a $\overline{P}$ regime) and not just to prices in a narrow commodity bundle (which, after all, is just a proxy). But even if the price level is more directly observable than the level of GNP, why is this an argument for $\overline{P}$ as opposed to $\overline{MV}$? It seems to me that the goal of policy should be, not to offer the public a display of stable prices, but to ensure that price-level movements that do occur reflect genuine changes in scarcity, so as to keep production levels as close to their natural rates as possible. This is what an $\overline{MV}$ regime would accomplish. If some consumers are bothered by natural-rate, price-level changes, it is only because economists have managed to delude consumers into believing that changes in the consumer price index (CPI) are more serious than changes in other price indexes.

Adverse Supply Shocks

Yeager focuses on adverse supply shocks, which he admits constitute the most "embarrassing" case against a $\overline{P}$ regime. The problem is that to keep the price level from rising as productivity declines, the money stock and nominal income must be contracted. Yeager sees "no logical or factual error" in this criticism; he even admits that a positive monetary stimulus to raise money income and the price level "might seem more sensible" in the event of an adverse supply shock than a $\overline{P}$ policy.

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What, then, is Yeager's reply? An accommodative policy, Yeager argues, would merely encourage producers to adopt rigid price policies. "Why," he asks rhetorically, should anyone "cut a price or wage if monetary expansion is likely to make the cut unnecessary?" Yeager's argument is invalid on two grounds. First, it attacks the straw man of "accommodative" monetary policy, erected by Yeager himself. Under the standard assumption of a unitary income-elasticity of demand for real money balances, an $\overline{MV}$ policy in the face of an adverse supply shock would (unlike an accommodative policy) not require any monetary expansion. Second, the argument proves too much, for if the goal is really to encourage price flexibility, neither a $\overline{P}$ regime nor an $\overline{MV}$ regime is ideal. A $\overline{P}$ regime encourages rigidity of output prices, whereas an $\overline{MV}$ regime encourages rigidity of factor prices. If we are to really encourage all-around price flexibility, we need a regime that is either highly inflationary or highly deflationary, as well as erratic. In other words, precisely the sort of regime that Yeager abhors. Score: I am tempted here to award the fly another point; but as the arguments just considered have to do with the issues similar to those discussed at the beginning of my comment, I will let the score remain at fly 1, hammer 0.

Yeager argues that to shape monetary policies and institutions "for the worst conceivable cases" (like adverse supply shocks) would be "perverse." But this view is true only if the chosen policies and institutions are inferior to other alternatives under more "normal" circumstances. An $\overline{MV}$ policy is (Yeager here again tacitly admits) superior to a $\overline{P}$ policy in the "worst case" of an adverse supply shock. He fails to note, however, it is no worse in any other case, so that it is not, after all, designed to serve just one "specific circumstance." Yeager states, "If . . . the balance of considerations favors institutions achieving monetary stability [meaning $\overline{P}$] . . . then it is simply irrelevant to think up particular cases in which some other price-level behavior might seem preferable." I quite agree, except that the balance of considerations—as revealed by our scoreboard so far—seems to favor institutions achieving $\overline{MV}$, not $\overline{P}$. Score: still 1–0.

Compatibility with Free Banking

Yeager thinks that a $\overline{P}$ policy is compatible with free banking, whereas an $\overline{MV}$ policy "must be pursued by a central monetary authority." I have argued (Selgin 1988, chap. 5, and 1990) that a free-banking system would automatically tend to stabilize nominal income. Among the requirements for this are (1) that the stock of base money be frozen or subject to gradual augmentation only and

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(2) that bank money (notes and checks) replace base money in payments apart from bank clearings. Historical gold-standard, free-banking systems like those of Scotland and Canada came close to meeting both requirements, but were prevented from fully meeting them by various legal restrictions. Thus, free banking seems to offer a practical and nongovernmental basis for implementing an $\overline{MV}$ regime. On the other hand, there is no evidence that Yeager's distinct vision of free banking would automatically emerge as a spontaneous consequence of laissez faire (including shutting down the Fed). Yeager's scheme could happen if enough persons attach a high value to having money of roughly stable purchasing power. Personally, I believe most people, anticipating a positive productivity trend, would favor money issued by $\overline{MV}$-norm free banks over money issued by $\overline{P}$-norm free banks because the former money would provide a higher overall real interest return to its holders.[2] I am speculating, though, so the score does not change.

Eliminating the Need for Forecasting

Yeager claims that his version of free banking "would radically reduce the need for forecasting." But would it? Recall that, in his system, the unit of account is based on a manageable bundle of commodities; stability of the unit is supposed to serve as a proxy for stability of the general price level. The unit's success in this regard depends crucially on a proper choice of bundle commodities: Some commodity prices will more closely follow the CPI than others. Implicit in Yeager's scheme, therefore, is the need for bank managers to forecast accurately future commodity price movements so as to exclude "deviant" commodities from their chosen bundles. To fail to do so would mean disappointing customers who (presumably) desire a money of stable overall purchasing power. Alternatively, the bundle might occasionally be redefined, but this would necessitate costly renegotiations of the terms for interbank settlements involving payments of some single-commodity "indirect" redemption medium.[3]

[2]This conclusion assumes that some bank money in either kind of system will consist of notes bearing zero nominal interest.

[3]I am assuming here that competing bankers can offer distinct units of account, so that the medium of redemption is also the normal unit of account for interbank payments. Alternatively, the government could define and redefine a unit of account to be used by all banks. Such a monetary system would not, however, be truly "nongovernmental." It would also be vulnerable to public runs for the medium of redemption whenever the public anticipated a devaluation of the unit of account in terms of the redemption medium. On this point, see Glasner (1989, p. 233).

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Perhaps the problem is not so serious as I imply—it may be that a narrow bundle that accurately tracks the CPI for a long period is easy to discover. Past experience, though, is not reassuring. A few years ago Robert Greenfield and Yeager (1983) endorsed Robert Hall's (1983, p. 115) suggestion of a bundle consisting of specified quantities of aluminum, copper, plywood, and ammonium nitrate. Hall chose this bundle, which he dubbed the ANCAP, "after studying the relation between the cost of living and the prices of a long list of suitable commodities." Alas, soon after Hall made his suggestion, the ANCAP and the CPI began to quarrel and then parted company for good.[4] This outcome might have posed quite a challenge to any $\overline{P}$-norm banker who, in 1983, elected to peg his money to the ANCAP.

An $\overline{MV}$-norm free banker would have no similar worries. Like any banker today, he would have faced some forecasting requirements concerning the future earnings of borrowers and the monetary preferences of the public, but he would not have needed to forecast or even think about the value of $\overline{MV}$ to help stabilize that value. All he would have had to do was to keep his reserves at their profit-maximizing levels. Stability of income would have resulted automatically as an unintended consequence of his and other free bankers' profit-maximizing behavior.

Final score: fly 2, hammer 0.

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[4]Unpublished statistics were provided by Fernando Alverez of Fairleigh Dickinson University.

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The Rational Expectations Revolution: An Assessment

Kevin D. Hoover

A Revolution?

My task is to write on the "Rational Expectations Revolution." I would not have chosen that title myself. One reason is that I have previously argued partly that the conception of the development of science as periods of "normal" problem solving punctuated by revolutionary upheavals is inappropriate to economics and possibly even to physics (Hoover 1991a). And yet, since Thomas S. Kuhn's *Structure of Scientific Revolutions* (1962), this conception has percolated down from physics through the physical and biological sciences finally to reach economics. I think there is more continuity and mutual comprehension between the putative revolutionaries and the *ancien régime* than Kuhn's account of scientific revolutions supposes. More than that, even though the rational expectations hypothesis is a critical element in its early development, the essence of what I prefer to call the "new classical macroeconomics" is found in the assumption that agents are consistent and successful optimizers, that all markets clear. The rational expectations hypothesis is a subsidiary theme in recent developments in the new classical school. That is not to say that I think the rational expectations hypothesis can be ignored: *liberté, egalité, fraternité* are still emblazoned on French coins even though they are subsidiary to the process and final outcome of the French Revolution.

Reservations to one side, I am not insensitive to the dramatic changes in academic macroeconomics over the past 20 years. Neither of the two schools of thought that dominate macroeconomics would

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be what it now is without the events surrounding the introduction of the rational expectations hypothesis into macroeconomics. (It is interesting to note that for its first decade even the new classicals identified themselves as "rational expectationists."[1] I will, therefore, indulge the revolutionary metaphor, recognizing, however, that the rational expectations hypothesis itself is not what will command our attention; instead we will examine the implications of those developments in macroeconomics for which the rational expectations hypothesis was one of the sparks. In the following impressionistic sketch of the history of those developments, I will pay special attention to the implications for forecasting and policy.

Tinder

The macroeconomics of the 1930s was a rich tapestry. The debates among Keynes, Hicks, Hayek, Hawtrey, Robertson, Robbins, Meade, Myrdal, and others were subtle and full of detail. Careful, thoughtful discussions of economic behavior and the role of expectations were prominent. Most of the debates were carried out in words and not in mathematics. Much of the richness of those discussions was lost in the subsequent developments. Keynesian economics, as it was known after the 1940s, especially in the United States, owes more to Richard Stone and Jan Tinbergen, the fathers of quantitative macroeconomics, than to John Maynard Keynes.[2] Keynes himself was deeply skeptical of Tinbergen's effort to construct a closed, empirical model of the economy (Keynes 1939). And the characteristic prescriptions of Keynesian macroeconomic policy—deficit finance, redistribution, and discretionary monetary policy—find precious little direct support in *The General Theory*.

The early Keynesian macroeconomics was cut from a plain cloth. Simple linear equations, estimated using elementary econometric techniques, connected a few macroeconomic aggregates. Simple forecasts and broad-brush analyses of policy were conducted. But economists were not content with these austere garments and began quickly to embroider them. First, macromodels grew in size and complexity: Where Tinbergen's model of the U.S. economy had 48 equations, the Federal Reserve's MPS Model has 334.[3] Second, economists began to explore the microeconomic behavior that was

[1]The first reference that I know of to a new classical school is in Chapter 16, "Aspects of the New Classical Macroeconomics," of Thomas Sargent's (1979) textbook.

[2]On the general need to distinguish Keynes from the Keynesians, see Leijonhufvud (1968).

[3]See Morgan (1990, p. 115) and Brayton and Mauskopf (1987, p. 93).

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presumed to lie behind estimated macroeconomic relations. The work of Milton Friedman and Franco Modigliani on the consumption function, of James Tobin and William Baumol on the demand for money, and of Don Patinkin on general equilibrium foundations for macroeconomics is typical of these developments.[4] Third, economists began to pay more careful attention to the processes through which expectations were formed. A first pass, found for example in Milton Friedman's (1957) theory of the consumption function or Phillip Cagan's (1956) work on money demand in hyperinflations, held that the expected value of a variable was formed on the basis of its history. Extrapolative expectations were empiricially tractable but theoretically unattractive because they were not obviously grounded in sensible economic behavior. By the end of the 1950s, serious research on the formation of expectations was under way. Herbert Simon and John Muth, both at Carnegie-Mellon University, developed divergent approaches.[5] Simon's work led to his theory of bounded rationality, while Muth's ended in the rational expectations hypothesis.

Muth noticed that dynamic economic models use expectations of variables as inputs into decisionmaking processes and, at the same time, generate predictions of the values of those same variables. If the model is correct and if people use the model to make predictions, then consistency should dictate that people should, in fact, expect (and use as an input into their decisionmaking processes) precisely what the model tells them will occur. For Muth, a rational expectation is one that is consistent in this sense. His insight has been refined in a number of ways: Expectations are rational if (a) they are formed as if people knew the true model, and (b) they are correct up to a serially uncorrelated error. That is, prediction errors are unsystematic. To take Muth's own definition, "[T]he subjective probability distribution[s] of outcomes tend to be distributed, for the same information set, about the prediction of the theory (or 'objective' probability distribution of outcomes)" (Muth [1961] 1981, pp. 4–5).

Muth used the rational expectations hypothesis to solve problems in the dynamics of agricultural markets. It was largely neglected for a decade. The honor of introducing it into macroeconomics belongs to Robert Lucas. Lucas is a scion of the monetarist class, which, at the time he was just entering his intellectual majority, thrived almost exclusively at the University of Chicago.

[4]See Friedman (1957); Modigliani and Brumberg (1954); Tobin (1956, 1958); Baumol (1953); and Patinkin (1965).

[5]See Sheffrin (1983, pp. 1–5) for a short account.

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Keynesian economics is founded on the notion that there are often unused resources in the economy. This notion is usually described as disequilibrium in the market for labor: There is involuntary unemployment; the supply of labor is greater than the demand for labor. The Keynesian economics of the 1950s was notably weak in accounting for the dynamics of inflation. Paul Samuelson and Robert Solow (1960) imported the inverse relationship between inflation and unemployment discovered by A. W. Phillips (1958) to fill the gap in the Keynesian model. The Phillips curve appeared to be an empirical fact; its slim theoretical foundations were found in labor market disequilibrium.[6]

Monetarism rejected the central basis of Keynesian economics. The economy does not have unused resources in equilibrium; and, in the long run, the economy returns to equilibrium. Inflation is "always and everywhere a monetary phenomenon." Monetarism is a doctrine steeped in irony. Its theoretical core is the quantity theory of money, which implies that, in the long run, money is neutral: Money is a veil, which masks, but does not alter, real resource allocation in a smoothly functioning economy. Yet, as the name "monetarism" implies, money is important. It is, however, only the pathologies of money that matter. For example, according to Friedman (1968), the observed Phillips curve is a result of workers not at first comprehending that higher money wages as a result of inflation were not also higher real wages; they suffered from money illusion. Once, however, they catch on, once their illusions are dissolved, the monetary pathology is cured and money is once again neutral.

Despite their differences, monetarist and Keynesian economics have an important common element: They are both accounts of the behavior of aggregates. Lucas and Rapping's (1969) article attempts to provide microfoundations for the monetarist labor market. In their article, employment is determined as an equilibrium; the supply of labor equals the demand for labor. The supply of labor, in turn, is derived from individual utility maximization, while the demand for labor is derived from profit maximization by firms. Although I have elsewhere (Hoover 1988, p. 27) referred to Lucas and Rapping's article as "surely the first paper to deserve to be called 'new classical,' " it is in an obvious sense prerevolutionary. Expectations are crucial in the underlying maximization problems; and Lucas and Rapping model them as *adaptive* expectations, not as rational expectations.

[6]On Phillips's own interpretation of the Phillips curve, see Desai (1975) and Wulwick (1987).

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Spark

Muth is the Rosseau of the rational expectations revolution; Lucas is its Robespierre.[7] The rational expectations revolution began with a series of papers in which Lucas worked out the implications of Muth's rational expectations hypothesis for the Phillips curve and the analysis of labor markets. Lucas's ([1972a] 1981) article, "Econometric Testing of the Natural Rate Hypothesis," will serve as the paradigm.[8] The article accomplished three things critical to the development of new classical macroeconomics.

First, it undermined the standard interpretation of the empirical evidence about the Phillips curve. A frequent test of the expectations-augmented Phillips curve or the natural-rate hypothesis before Lucas was to check whether the sum of the coefficients on lagged inflation summed to zero in a regression of unemployment (or output) on inflation. The idea was that the economy tended to return to full employment equilibrium (the natural rate of unemployment) if an inflation (money) shock washed out over time. Lucas showed that one should expect such a result only when expectations are rational if monetary policy was purely random. If policy followed any kind of dynamic rule (say, a constant growth rate of money), then people's expectations of the future course of money, based on their internalization of that rule, would imply coefficients that do not, in general, sum to zero even when *ex hypothesi* the natural-rate hypothesis is true. Tests that seem to reject the natural-rate hypothesis might, in fact, be providing evidence for it, so long as expectations are rational.

The second accomplishment of Lucas's article was to shift the analysis of policy away from single policy actions, such as were frequently analyzed in the IS/LM framework, to policy rules. The point is partly ideological, an expression of the Chicago commitment to rules over discretion (or authorities) that goes back to Henry Simons (1936). It is, more importantly, a product of the logic of rational expectations: Any policy, whether intentionally rule based or not, can be divided into a systematic component (upon which rational predictions may be based) and a random component (which is pure noise). The noise, although it can lower the efficiency of the economic system by interfering with relative price signals, cannot be useful for policy, because it is unsystematic. The systematic

[7]I, of course, would not push the analogy too far. In particular, I would not attempt to detail a new classical "terror," although there are perhaps a few macroeconomists who think that it could reasonably be done.

[8]Hoover (1991a) gives methodological reasons why it may not matter very much which of many models or articles one takes to be the paradigm.

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component can be integrated into the decisions of firms and individuals. Policymakers cannot ignore that there is an interaction between their policy rules and individual behavior. They must account for such interactions in the design of policies.

The third accomplishment of Lucas's article was to undermine the structural interpretation of estimated econometric models. Keynesians had regarded the Phillips curve as a structural relationship. But Lucas showed that, under the rational expectations hypothesis, the coefficients on lagged inflation depended on the policy rule in place. Thus, when the rule changes, the coefficients themselves must change.

Conflagration

From the initial spark of the rational expectations hypothesis, a great fire raced through macroeconomics, consuming almost nothing completely, but singeing everything. The three great themes of the new classical macroeconomics in the 1970s—policy ineffectiveness, dynamic consistency, and the Lucas critique—were the natural extensions of concerns expressed in Lucas's article.

Policy Ineffectiveness

Only the systematic component of policy could be systematically directed toward the policymakers' ends. Under rational expectations, people could not long mistake the systematic thrust of a policy. Therefore, if, as monetarism maintained, the real economy had a tendency to equilibrium, policy could not drive the economy away from that equilibrium. Monetarism implied that only money illusion gave monetary policy a real effect; the rational expectations hypothesis implied that money illusion was fleeting at best.[9] The famous articles of Sargent and Wallace (1975, 1976) make this obvious point. Most of the attacks on the policy-ineffectiveness proposition centered on the natural-rate hypothesis and not on the rational expectations hypothesis (e.g., Fischer 1977 or Phelps and Taylor 1977).

Dynamic Consistency

The policy-ineffectiveness propositions were concerned with the efforts of policymakers to boost output or to lower unemployment (i.e., with the real economy). They worked because the public integrated the policy rules into their own decisions. The rules themselves

[9]I am ignoring, as does this literature, the beneficial effects identified by Friedman (1969) of a policy of systematic deflation or by Tobin (1965) and Mundell (1971) of a policy of systematic inflation.

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were autonomous—independent of the actions or reactions of the public—and were treated as if, once set, they remained in place for all time. The new classicals extended the analysis of rules: First, the policymaker was treated symmetrically with the public—policy rules were analyzed as the outcome of an optimization problem; second, the range of policymakers' concerns was extended beyond the real economy to include an aversion to inflation;[10] and third, rules were analyzed in richer dynamic settings. To take the last point first, Finn Kydland and Edward Prescott (1977) pointed out that, when choosing an optimal rule in a dynamic setting, policymakers may be subject, in addition to feasibility constraints, to a requirement that policy be dynamically consistent. For example, suppose that, to maximize one's child's welfare, a parent proposes the policy that, if the child comes home after midnight, her legs will be broken. If, in fact, the child appears after midnight, such a policy would hardly seem optimal at all, for the child with rational expectations sees no cost in staying out past midnight. The policy is dynamically inconsistent. Robert Barro and David Gordon (1983a, 1983b) *inter alia* analyze monetary policy in an environment in which the policymakers lose utility from both higher unemployment and higher inflation. Permanently positive inflation may result in such a model because, even though inflation cannot permanently decrease unemployment, it is not dynamically consistent for the policymakers to eschew the temptation to attempt to fool the public for a gain of temporarily lower unemployment.[11]

The Lucas Critique

Lucas (1976) generalized the message of his "Econometric Testing of the Natural Rate Hypothesis": Aggregate, econometrically estimated relationships are not, in the presence of rational expectations, invariant to changes in policy regime. The point, however, goes beyond the rational expectations hypothesis. In general, if people are optimizers, the higher-order implications of their optimization problems are not likely to be invariant to changes in the environment. Invariance can be secured only if one gets down to the level of the underlying optimization problem, taking only tastes and technology as given.[12]

[10]Exactly why inflation should be seen as costly in typical new classical models was unclear since money was essentially neutral.

[11]See Rogoff (1989) for a review of the literature.

[12]It is difficult to know where one can stop, since both tastes and technology have been analyzed as objects of choice rather than as primitives.

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The upshot of these three themes for forecasting is devasting: Using large-scale macroeconomic models to make conditional forecasts makes no sense. First, these models are, at best, incorrectly specified, because they do not consistently integrate rational expectations, even at the aggregate level.[13] Second, policy experiments are incorrectly conceptualized in them—issues such as dynamic consistency were not faced. Finally, they simply are not accurate representations of economic structures that would remain invariant to changes in the policy regime; therefore, they could not be used to forecast changes that are conditional on a new regime.

From Republican Virtue to Faction

The new classical macroeconomics self-consciously embraces a foundational myth, laying claim to the legacy of pre-Keynesian neoclassical economists (see, e.g., Lucas [1977] 1981, pp. 215, 217; [1980] 1981; and 1987, p. 47). It seeks to erect a republic of virtue on the principles of an earlier age—a macroeconomics that is strictly based in the microeconomics of resource allocation. The implications for forecasting are grave. Consider two types of forecasts. People adjust their behavior to weather forecasts (they take umbrellas to work and they cancel picnics), but their behavior does not change the weather. On the other hand, when water-use forecasts suggest that demand may outstrip supply, people often adopt conservation measures that will ensure that actual outcomes diverge from the initial forecast—indeed that is the point of the forecast. Keynesian macromodels implicitly assumed that their forecasts were like weather forecasts: All agents (except, notably, the government) could treat the forecast as independent of their own behavior. The new classical economists explicitly assume that all forecasts are like water-use forecasts. Recognizing this distinction suggests severe difficulties in applying standard econometric techniques to macroeconomic models. The new classical macroeconomics has fractured into factions defined by their methodological response to these problems.

Lars Hansen and Thomas Sargent ([1980] 1981) and others treat the problem as a failure of econometricians consistently to enforce the implications of rational expectations in their estimation of large macromodels. Thus, they attempt to answer the Lucas critique by deriving econometric specifications from underlying optimization problems, taking only tastes and technology as given. They then

[13]It should be recalled that Sargent and Wallace's policy-ineffectiveness results were derived in an IS/LM model with rational expectations, and not in a model with consistent microfoundations.

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estimate these equations using extensions of standard econometric techniques that account for the nonlinearities introduced by rational expectations (the cross-equation restrictions). They appear to be happy if their models are not, in Sargent's memorable phrase, "obscenely at variance with the data" (Sargent [1976] 1981, p. 547).

Obscenely or not, models of the Sargent and Hansen type are almost always at variance with the data. Kydland and Prescott (1990) and Lucas (1987) regard this as the inevitable result of the failure of the microeconomic theory to map properly onto the macroeconomic data. The world is too complex for a model to possess both sound theoretical foundations and sufficient detail to describe data accurately. Where Sargent and Hansen attempt to save the project of macroeconometric modeling through improved technique, Lucas and Prescott wish to adapt the viewpoint of the engineer to save policy analysis.[14] They see models as schematic representations that, while not realistic, are nonetheless similar enough in key dimensions and, above all, are tractable so that the implications of a policy can be worked out in all relevant detail. Lucas and Prescott see Sargent and Hansen's approach as impossible to implement; Sargent sees Lucas and Prescott's approach as empirically unsupported.[15] Where Sargent and Hansen attempt to perfect the forecasting enterprise, Lucas and Prescott give it up altogether.

A third faction saves the traditional macroeconometric models at the expense of their structural interpretation. Christopher Sims (1980, 1982, and 1986) argues that if policymakers are part of the economic process, so that policy is endogenous, then in some sense there are no changes of regime. Rather, what appear to be regime changes are just different realizations of a stable supergame.[16] Thus, while the Lucas critique is correct in principle, the coefficients of macromodels rarely change, and the models may be used for forecasting. The only problem is that the reason one wants to have forecasts in the first place is to guide changes of behavior. Here forecasts are valid, but only because they do not induce changes in behavior. Macroeconomic forecasting models are saved; policy is emasculated.

[14]One should not be misled by the fact that the business-cycle models of Kydland and Prescott do not model policy at all. These are prototypes of models that are meant to be used to address policy issues—this much is evident from Prescott's methodological musings. Lucas's (1987) calibration exercise, assessing the costs of the business cycle, is clearly policy evaluation.

[15]A fuller discussion of this debate is found in Hoover (1991b).

[16]This overstates Sims's position. He actually says that regime changes are rare, not unknown. Cooley, LeRoy, and Raymon (1984a, 1984b) take a starker view. See Hoover (1988, chap. 8, sec. 8.4) for an extensive account of the issues.

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As well as factions based on methodological difference, the new classical macroeconomics is also fractured over issues of substance. A principal goal of new classical research since the mid-1970s has been to provide a satisfactory account of business cycles. The early business-cycle models of Lucas ([1972b] 1981, 1975) and Barro (1976) were monetary models: Unanticipated changes in the money stock initiated the cycle, while the dynamics of capital-stock adjustment propagated it over time. In the late 1970s, empirical investigation of unanticipated money as the driving variable in business cycles held the center stage in macroeconomic research.[17] Although there has been no absolutely decisive result, the weight of evidence is now against unanticipated money as the source of cyclical fluctuations. Real-business-cycle models (models in which the source of cyclical fluctuations is shocks to technology) now predominate. Latent monetarism, however, keeps some new classicals searching for alternative ways to model the influence of money.[18]

Legacy

The whole of academic macroeconomics is touched by the rational expectations hypothesis. Although it has been criticized on conceptual grounds and as empirically inadequate, the rational expectations hypothesis sets a noncontroversial standard for modeling expectations in macroeconomics.[19] Alternative approaches are either out of the academic mainstream (e.g., approaches based on Herbert Simon's notion of bounded rationality) or are positively shunned (all adaptive and extrapolative approaches). The rational expectations hypothesis holds the field by default. Beyond this, the rational expectations revolution was accompanied by, and to some extent caused, an increase in the technical sophistication of academic macroeconomics. Whether or not one thinks this to be a good thing, a comparison of randomly chosen articles in macroeconomics from journals circa 1965 and 1990 will confirm a revolution in formalization.

Still, for all of the upheaval in academic macroeconomics, practical forecasting and policy analysis have been largely untouched. When Congress, the Federal Reserve, the Treasury, corporations, or newspapers want a forecast, they turn to macroeconomic models (larded, of course, with a dose of professional judgment) that are only marginally

[17]See, for example, Barro ([1977] 1981, [1978] 1981); Mishkin (1983). Hoover (1988, chap. 8, sec. 8.2) summarizes the evidence.

[18]See, for example, Lucas (1987, secs. 6 and 7).

[19]There are, of course, some critics, for example, Lovell (1986).

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different from those of the late 1960s. Part of the problem is tractability. It is simply very difficult to impose the full implications of the rational expectations hypothesis on models like the Fed/MPS model or the DRI model.

Beyond that, however, the very nature of new classical models reinforces the divergence between practical and academic macroeconomics. Those in government and business usually want to know what next year's GNP or interest rates will be or, at best, what the path will be over some short horizon. To the new classicals such questions are wrongly posed, and their models are not adapted to answering them. Business cycle models are constructed around neoclassical growth models. Typically, it is easier to get answers about "steady states" than short-run paths from such models. Trend rates of growth, or for that matter the levels of real variables, are often modeled as beyond the influence of policy. If the models have money, money is neutral. Most importantly, where decisionmakers generally consider policy actions, the new classical macroeconomics insists on evaluating policy rules. Real-business-cycle models are rarely estimated. Instead, they are simulated with repeated realizations of their random shocks. The output of such simulations is never a forecast of a dated value of a variable. Rather, it is a set of variances and covariances between variables that reflect the operating characteristics of a policy rule. Such models answer a question that decisionmakers rarely ask.

The new classicals have, of course, not had the field entirely to themselves. There is a Keynesian counterrevolution. The new Keynesians revel in the increased technical level of the past 20 years. The rational expectations hypothesis is not a point of controversy. And although they too are no longer involved with large macroeconometric forecasting models, they do take seriously the message suggested by those models: Markets do not clear, sometimes for relatively long periods, and the performance of the economy is often suboptimal. This message is the basis for an academic attack on the new classical macroeconomics. The attack is conducted largely in the style of microfoundations of macroeconomics (i.e., for market failures, it seeks explanations that are consistent with individual optimization). The attack has four main threads.

First, prices or wages are sticky so that increases in aggregate demand are reflected partly in output and employment and are not absorbed entirely in price increases. Justifications of sticky prices or wages include the existence of wage contracts, quality of labor that depends on the wage paid, and costly price adjustment.[20] Second,

[20]See, for example, Phelps and Taylor (1977); Fischer (1977); Akerlof and Yellen (1985,

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imperfect competition may produce suboptimal levels of output and employment.[21] Third, economies of millions of people may have difficulties coordinating economic activity in the manner of the Walrasian general equilibrium models preferred by new classicals. Coordination problems may lead to multiple and self-sustaining, but suboptimal, equilibria.[22] And fourth, the monetary system, as opposed to narrow definitions of money, may not be neutral. The problems of financing real activities on credit markets subject to their own upheavals may contribute to the suboptimal use of resources.[23]

The fourth point carries a final irony: For years monetarists insisted against some Keynesians that money matters; now the Keynesians are the ones to insist against many new classicals (whose roots were in monetarism) that money matters.[24]

Republicans and Monarchists

My broad-brush history of the new classical macroeconomics has not been a particularly hostile one. Yet, I think it is easy to see any number of conceptual, methodological, and ideological grounds on which one might oppose the new classical macroeconomics. My suspicion is that there are simpler grounds for opposition; namely, many of the key propositions of the new classical economics are simply not true: Expectations are not, in fact, rational; markets are not perfectly competitive and do not clear; both the policymaker and the public play a pretty unsophisticated game; money is not neutral; and technology shocks do not drive the business cycle. But the truth is rarely simple. So I expect that just as France on the 200th anniversary of its revolution remained divided between supporters and opponents of the revolution, so macroeconomics will long debate the implications of the rational expectations hypothesis and the modes of analysis that followed in its wake.

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1986); Mankiw (1985).

[21]See, for example, Blanchard and Kiyotaki (1987).

[22]See, for example, Cooper and John (1988).

[23]See, for example, Greenwald and Stiglitz (1988); Bernanke (1983).

[24]See, for example, Romer and Romer (1989).

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Rational Expectations and the New Classical Macroeconomics

David I. Meiselman

A Unified Theory

There has been a major change in the content, emphasis, and style at the frontiers of macroeconomics and monetary theory. The result is a sharp paradigm shift in the larger body of economic theory that has largely destroyed the old academic distinctions between macroeconomics and microeconomics. Now, for better or worse, we essentially have one economic theory again.

I emphasize the word "theory" because there is not yet much evidence that the new body of analysis has had much effect, apart from its impact on academic research and teaching. It has essentially left untouched the substantive macroeconomic and monetary questions raised on Wall Street; or on public policymaking inside the Washington Beltway or elsewhere; or on the production, consumption, and authority of economic forecasting exercises. By contrast, the rational expectations revolution and its offspring, the new classical macroeconomics, have had a profound effect on academic teaching, research, and journal publications. One need go no further than skimming the titles of articles in the leading academic economics journals for convincing evidence of the major change.

Demise of the Keynesian System

The rational expectations revolution has cleaned up much of the shambles left by the earlier, now defunct, Keynesian revolution. In retrospect, it should now be clear that much of the Keynesian revolution was so fundamentally and fatally flawed that Keynes

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himself would surely have repudiated it.[1] These exercises and efforts to explain short-period economic disturbances and business fluctuations mocked the very foundations of economics by ignoring its central analytical core, which is derived from utility and profit maximization, market pricing, and market-clearing processes. Those processes include the discounting of actual or anticipated future events such as prices, incomes, interest rates, and public policies. Thus, expectations of future events are central and necessary to the discounting process.

In his intellectual history of the demise of the Keynesian system, Kevin Hoover (1992) emphasizes the empirical weakness of the Keynesian theory and Keynesian models in periods of inflation such as the 1970s, as well as the presumed inflation-unemployment tradeoff of the Phillips curve, which was an adjunct of the standard Keynesian model. But Hoover overlooks the fact that Keynesian theory never really had any predictive content in earlier periods. Almost 30 years ago, well before the inflation of the 1970s and 1980s, Milton Friedman and I (1963) used U.S. data going back to 1897 to demonstrate that the standard Keynesian model was essentially empty. Similar studies were replicated for many other countries and periods and had essentially the same results as our original research. It was not the stagflation of the 1970s that destroyed the Keynesian model and exposed its shortcomings; the model had no substantive or predictive content either before the 1970s or since.

A New Macroeconomics

The present state of macroeconomics and monetary economics leaves many people in a puzzled and dissatisfied state. The rational expectations critique and the new classical macroeconomics were major factors in destroying the old Keynesian macro theories that relied on haphazard, non-optimizing rules of behavior and on presumed and untested empirical rules of thumb. Instead, economists in the rational expectations and the new classical macroeconomics traditions have tried to base a new macroeconomics on standard elements of microeconomics. Those economists have used both

[1]Some years ago in a paper I wrote with Paul Craig Roberts (1979) evaluating the Congressional Budget Office's Keynesian-based forecasting model, I showed that the standard Keynesian model featured "demand without supply, inflation without money, interest rates without capital, outputs without inputs, employment without wage rates or a labor market, and investment without saving or any change in the capital stock." In that model, expectations were static, consumption depended only on current disposable income, and fiscal policy had no supply-side effects.

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rigorous price theory and rigorous statistical and econometric tools in evaluating evidence.

Many results of the rational expectations critique have been devastating to formerly received doctrine. Yet, these analyses have little to offer public officials or private sector businesspeople and investors who continue to ask the same serious questions about making and using short-run economic forecasts and about the short-run effects of various public policies, including so-called stabilization policies. Also, the new models generally assume a closed rather than an open economy. They neglect influences to and from abroad—a serious omission, particularly in a world increasingly open and integrated, especially in financial markets.

Impact on the Quantity Theory of Money

By contrast, I do not believe that the rational expectations revolution has had the same dire impact on the quantity theory of money, the old major alternative to the Keynesian model. First, monetarism never attempted to spell out a complete or general equilibrium model for the economy as a whole. Instead, it focused on the role and impact of monetary change and on the determination of the value of money—no small or unimportant chore. The quantity theory distinguished between the effect of changes in the stock of money on real variables in the short run and its neutrality in the long run. The long-run neutrality of money stemmed from the inability of nominal money to systematically affect real variables—apart from its pathology that imposed various deadweight welfare and real income losses. In the long run, with a lag, the stock of money generally affected only the price level. Also, the lags in the effect of monetary change were understood to be both long and variable.

To be sure, some monetarists, both at the St. Louis Fed and elsewhere, responded in part to the demand for forecasts and desired also to present forecasting alternatives to widely cited Keynesian-based forecasting models. Thus they created and generally believed their short-run forecasting models, which, for a time, successfully competed with and even outperformed the Keynesian-based models. Moreover, these monetarist-inspired exercises typically found that monetary change was destabilizing in the short run and neutral in the long run.

Monetarists always distinguished real values from nominal ones. Real variables and relative prices, including fiscal policy tax and government expenditure variables, determined resource allocation and economic growth. The best possible monetary policy kept money

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from causing inflation or deflation and from adding to short-run instability. Even the very best monetary policy could not alleviate or cure problems of poverty, of slow or no economic growth, of retarded technological change, or whatever. Indeed, use of monetary policy to affect real variables, relative prices, and interest rates could not systematically alter those real variables; it could only add monetary disturbances, which often had the perverse effect of making worse the supposed problem that more (or less) money was intended to alleviate. For example, as we all know now from much painful experience, trying to lower interest rates by rapid money growth not only failed to achieve lower interest rates, but ended up causing still higher rates.

The quantity theory was never intended to be a general equilibrium model. It focused on only a few important phenomena such as the long-run price level and short-run economic instability. The full range, precise content, and dynamics of monetary change were generally not fully articulated, explored, or tested. The quantity theory separated real effects from nominal ones, except that short-run monetary disturbances were empirically associated with real short-period business cycle disturbances. In the long run, nominal money affects only the price level. Real variables, including real interest rates, depend solely on real, not monetary, variables. In other words, money is neutral in the long run.

The quantity theory is in the Marshallian partial equilibrium tradition, not the Walrasian general equilibrium tradition. It proceeds on the basis that we do not need to know much about everything in order to predict and understand much more about a few important variables such as the price level.

There is merit in efficient use of appropriate abstractions in building and using theory (and elsewhere). It is costly to increase the number of variables and the range of phenomena to be considered, and the added costs are not necessarily balanced by the added benefits of increased precision or by the broader range of implications.

Indeed, in his book, *The New Classical Macroeconomics*, Hoover (1988) has an interesting analysis of important differences between the Marshallian partial equilibrium approaches of Milton Friedman and most other monetarists and the general equilibrium Walrasian approaches of many rational expectations and new classical macroeconomic theorists. The practical difficulty, or impossibility, in testing anything other than relatively simple models with relatively few variables argues for the Marshallian approach in empirical matters. Constructing theories that have readily testable content is a desirable goal. However, it would seem that the increasing complexity,

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formalization, and mathematization of macroeconomics and monetary theory in recent years have often made models less (rather than more) amenable to empirical testing and verification.

Although the rational expectations critique has been highly useful in questioning older, flawed theories and in raising valid questions about the many and varied tinkerings with monetary and fiscal policies, it is not clear that the rational expectations hypothesis and new classical macroeconomics have contributed a usable alternative to the quantity theory, particularly in a world with national domestic economies open to international trade and international financial transactions. To be sure, emphasizing the role of expectations, the consistency of expectations and underlying theory, the dependence of expectations on the policy regime, and the simple fact that people learn from experience have all been important, even if not always quite as novel or as innovative as some would have us believe.

The Problem of Testing the New Models

Despite the important analytical role given to expectations, we have made little progress in developing fruitful, testable, or tested models of expectations formation. It is not enough to assert that expectations are or ought to be consistent or generally unbiased over the long pull. Such an assertion is a far reach from a body of validated knowledge of how, in fact, expectations are formed and how economic agents and markets act on the basis of these expectations. This knowledge is generally necessary to independently test theories of how economic agents act on the basis of expectations, or how people respond to policy and other disturbances that alter expectations and behavior on the basis of those expectations. Validated knowledge of expectations formation is also required to test central elements of the signal extraction problem and process that has such a prominent role in much of the rational expectations literature. Moreover, trying to bypass expectations by assuming perfect foresight in a world otherwise characterized by uncertainty may yield interesting analytical results but is typically empty of substantive ones.

In my judgment, these approaches are likely to be sterile. Indeed, they are sterile without better—much better—tested theories of expectations formation. There is surely more to analysis that either critiquing the sins and errors of other theories or working out the formal, theoretical properties of increasingly complex systems.

Incorporating Public Choice Theory

Optimization behavior by private economic agents on the basis of utility or profit maximization—the essence of economics—is one

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thing. But what of governments and policymakers? What are they trying to maximize or optimize? It is surely both naive and wrong to believe that (a) governments and bureaucrats are idealists or saints devoted to optimizing a time-consistent social welfare function that maximizes economic growth, minimizes inflation, and the like; or that (b) governments and bureaucrats have the information and knowledge to do so. Such beliefs ignore both the multiple, conflicting, and often inconsistent pressures on governments and the multiple, conflicting, and inconsistent policies that governments typically pursue. These policies reflect, in part, the range of diverse pressures on governments and the interests of their diverse clients, as well as the separate self-interests of governments and bureaus, per se, and of their personnel. This non-idealistic view of governments—that governments and bureaus have their own self-interests—is part of the essence of the whole field of public choice, which largely seems to have been neglected in the public policy modeling in the rational expectations tradition. This oversight is, indeed, a serious defect. If we must use the best analytical, mathematical, and statistical tools to analyze private choice and market decisionmaking, should we not use the best tools for analyzing nonmarket decisionmaking by governments, bureaus, politicians, and legislatures in the formation of public policies?

Much of the policy ineffectiveness and dynamic inconsistency discussion in Hoover's paper never treats governments and bureaus as having any self-interest goals for themselves, their clients, their politicians, or their personnel. This serious flaw is not only in the Hoover paper but is in the entire body of analysis that the paper surveys.

Several years ago, I pointed out that most of the policy ineffectiveness analysis of the rational expectations approach proceeded on the basis of essentially a one-person game, where the central bank, attempting to achieve stabilization goals, adopted repetitive but unannounced strategies (Meiselman 1986). Like the proverbial sampling from a fixed urn, through time the public would learn of such unchanging strategies. Then, by anticipating central bank actions, à la rational expectations, the public's behavior would tend to undermine or eliminate policy surprises and, thereby, the real impacts of such strategies.

The true contents of the policy urn were assumed to remain the same. Thus, so-called stabilization actions came to be essentially known ahead of time and discounted. The result was that actual actions of stabilization agencies neither imposed surprises nor provided new information. In the long run, these policies were rendered

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ineffective as were the agencies operating such policies—hardly comforting words for the Federal Reserve whose chairman is now widely regarded as the second most powerful man in Washington.

To protect and enhance such power, as well as the role of the institution and its personnel, the Fed and other discretionary authorities cannot and will not commit institutional suicide by adopting a fixed strategy, including any stabilization or monetary policy rules. Indeed, the Fed will not adopt such rules and will fight, as it has fought, any attempt to have rules or fixed strategies imposed on it that would make monetary policy predictable. Instead, the Fed's institutional incentives are to adopt a strategy that permits it to continue to impose surprises on the public, and thereby to retain its power and to have real effects in the short run on income, employment, interest rates, and so forth. One further important by-product of this strategy is the impairment of accountability.

The changing and changeable Fed policies and operations keep those policies and operations essentially hidden from the public. The result is that Fed policies and operations are essentially unforecastable and undiscountable. Expectations of Fed policy may be rational in the sense that they are not systematically biased, but such expectations are typically so far off the mark that large numbers of people typically, consistently, and inevitably make large forecasting errors. Even the best and the brightest, including Nobel Prize winners who devote much of their professional lives to studying the Fed, have poor records forecasting Fed actions. In fact, if Nobel Prize winners, or others, become able to forecast Fed policies, the Fed can easily change its policies to keep itself unforecastable and still capable of imposing surprises. Thus the Fed remains both powerful and effective in imposing surprises.

Analogous to the Heisenberg principle in physics, when the public observes the Fed, that observation causes a change in the Fed's behavior. Similarly, when the public uses those observations to anticipate the Fed's actions, that changes the public's behavior, too. The result is unstable, dynamically inconsistent expectations of each other held by both the Fed and the public. Policies and actions chosen today as optimal, given today's expectations about tomorrow, may no longer by optimal when tomorrow comes because policies, actions, and anticipations can and frequently do change in unpredictable, essentially unforecastable ways.

One result is essentially "bilateral dynamic inconsistency," which is a process of sequentially shifting anticipations, behavior, and uncertainties of *both* the public and the Federal Reserve stemming from discretionary and unconstrained policy. Another result is that

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otherwise stable empirical relations and dependable lags in the effect of monetary change are likely to be disrupted and altered. Under discretionary monetary policy, because the underlying structure keeps changing and in ways that essentially cannot be specified, monitored, or measured, empirical links become less dependable and observed lags tend to become more variable. It is as if white noise or a random component were added to each variable. All become more difficult or impossible to isolate and to quantify. Underlying systematic relationships become blurred or buried.

The irony is that the Fed cannot hope to be credible if it fails to make itself effective in achieving stabilization goals, and the Fed cannot be consistently effective in achieving stabilization goals if it makes itself unpredictable. The Fed cannot avoid the genuine dilemma.

Discretionary policy essentially generates a two-person rather than a one-person game. Because the Fed cannot know what expectations the public holds or how the public's expectations will respond to the Fed's actions, the Fed's policies, too, are unavoidably subject to error in achieving Fed goals. Because the public cannot know what the Fed will be doing, the public, too, has more uncertainty and makes more errors in evaluating present and future markets and in making appropriate choices and decisions. Discretionary monetary policy cannot be rendered wholly ineffective by the public, because the public cannot determine with confidence what current policies are or what future policies will be. Monetary policy continues to generate uncertainty and dead-weight losses that have real short-run and long-run effects. Fine-tuning contains the seeds of its own self-destruction. Only in the world of a hypothetical, once-for-all change in money or of similar experiments would such policies have no permanent or no long-run impact on real variables, uncertainty, and economic welfare. In this sense, discretionary monetary policy is inconsistent with the long-run neutrality of money.

Policy Consensus

Most of us—old monetarists, middle-aged rational expectations theorists, and many new classical macroeconomists—come out with essentially the same policy conclusions. Our surprising and welcome consensus includes the following:

- Rules rather than authorities.
- Slow, steady, predictable growth of money.

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- Other policies consistent with a monetary rule, such as nonintervention in markets for foreign exchange, securities, or interest rates.
- Stable, predictable tax and expenditure programs.
- A goal of zero inflation, or at least an avoidance of the deliberate use of inflation to solve or cover up other problems such as unemployment, a weak housing market, and the like. In other words, we would shun the Phillips curve snare or the delusion that monetary change has dependable and controllable real effects.

All of this sounds like much of the original Reagan program. The Reagan administration never really pursued those policies, but for a brief period many Reagan officials discussed them.

A Learning Process

The rational expectations hypothesis and new classical macroeconomics have been faulted for lack of answers or involvement regarding some traditional questions of macroeconomics dealing with the analysis and forecasts of short-period business conditions and short-period stabilization policies. With some justification, rational expectations people respond by saying that (a) the old macro models may have tried to do so but could not forecast, either, and that (b) discretionary, short-period monetary and fiscal policies also did not work as planned. Instead, discretionary, short-run policies often caused long-run problems of inflation, bloated government, and resource misallocation. So, whatever the change of emphasis, by trying to frame the questions properly and to answer them on the basis of rigorous microeconomics, there is the potential for finally arriving at answers to fundamental questions regarding economic fluctuations, business cycles, and appropriate public policy.

If we examine some of this criticism, we see that it is as if researchers in cell biology and genetics, after having made significant progress in understanding cell and tissue physiology, were faulted because they had not yet found definitive answers to the causes of cancer or had not yet developed a cancer cure. But surely the preliminary and incomplete state of their knowledge is no justification either for stopping the fundamental research; for continually resorting to witchcraft, magic, and incantation; or for prescribing medications that poison rather than cure. In similar manner, the not wholly satisfactory state of the new understanding of macro phenomena does not mean we should discontinue our search for knowledge.

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Perhaps some day we will know more of both pathologies and their cures.

Conclusion

My comments are not meant to be critical of Hoover's scholarly paper, which is an excellent summary and critique of major and fundamental changes in macroeconomics and monetary theory in the past 20 years. His paper and, even more, his book, *The New Classical Macroeconomics,* are excellent guides to the literature and to the issues. Both works are well written, finely nuanced, and remarkably fair in an area of economics usually dominated by shrill acrimony.

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Private Forecasting and Government Policy Stability

Jerry L. Jordan

The Meaning of Forecasting

To forecast and to predict are not necessarily the same thing. In my dictionary, the first definition of forecast is "to form an opinion beforehand"; the first definition of predict is "to tell in advance." But my dictionary goes on to say that forecast and predict are synonyms. I do not use them that way. Also, what some people call forecasts are nothing more than assumptions that they act on, whether they would assign a high probability to certain events or not. There are occasions when it makes sense to base business or personal investment decisions on an assumption about the future that one does not necessarily consider to be the most likely. During the Middle East crisis and the war with Iraq, it made sense for business decisions and personal investing decisions to be based on a worst case about the macroeconomy even if that case was considered to have a low probability. There is, after all, a logic to minimax strategies.

In some sense, both forecasting and policymaking are inevitable. However, forecasting by government is not inevitable, and policymaking need not be activist. Avoiding swings in nominal GNP is desirable, but stabilization policies may not be stabilizing. Stable exchange rates are desirable, but actions by government to stabilize them may not achieve that result.

Private Forecasting

With apologies to Descartes, "I think, therefore I plan." Individuals make current decisions that have future consequences and in

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doing so behave *as though* they could foresee the future. Most business decisions and personal investment or consumption decisions result in actions that involve at least implicit forecasts about the future macroeconomic environment. Since choices often involve a time dimension, the tradeoffs are subject to change under alternative assumptions about such factors as the purchasing power of money, interest rates, exchange rates, and tax rates.

We make decisions; therefore, we do forecast. Because change is inevitable, forecasting is inevitable. The only thing certain about the future is that it will not be the same as the past. Individuals have expectations and beliefs; therefore, they are all forecasters. In this context, forecasting is a positive (non-normative) activity. A private forecast does not suggest desirability nor does it involve preferences about the future.

In contrast, policy is *normative* on the part of the policymakers. So-called forecasts by government authorities are not comparable to those made in the private sector. Consequently, *ex post* analyses that purport to compare the accuracy of private- and public-sector forecasts of macroeconomic variables are silly.

It is important to distinguish between assumptions about the future for certain purposes—budgets, planning, policies—and predictions about what is going to happen in the future. Private-sector forecasting involves identifying various possible states of the world in the future and assigning probabilities to them.

A major part of the exercise is seeking internal consistency between assumptions or beliefs about governmental policy actions and the logical consequences. To predict in February which team will win the World Series next October is not similar to a macroeconomic forecast. Deriving the implications for various teams or players of changing the height of the pitcher's mound or implementing the designated hitter rule is similar to what private forecasters attempt to do.

The economists' general equilibrium framework incorporates feedback effects from the numerous microdecisions to the macroimplications. It would be true that I could see better by standing up in the football stadium only if my behavior did not influence the behavior of others. Given that my behavior is likely to affect others, it is not as dubious as it sounds to say that some fans can see better sitting down than standing up.

Governments and Forecasting

For private-sector macroeconomic forecasters, one of the greatest sources of uncertainty about the future is the effects of government

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policy actions. Often it is not known what policy actions various governmental bodies will take. However, even when actions have already been taken, there is uncertainty about the timing and the magnitude of the effects.

The least useful piece of information to the private forecaster is the intent of the policymakers. It is helpful to distinguish between *policies* as statements of policymakers' intent and *policy actions* as what is actually done. Such terms as "easing" or "tightening" often represent nothing more than an expression of intent. Actions to achieve intended conditions may have quite different effects.

If what we refer to as "government" constituted no more than a set of rules and contractual arrangements according to which private decisionmakers interact with one another, then the nature of macroeconomic forecasting would be different from what we now observe. However, where government agencies take actions (such as spending; taxing; open-market operations; reserve requirements; foreign-exchange interventions; and wage, price, credit, capital, or exchange controls), the private-sector forecaster must take account of the probability of such actions and their likely consequences.

Following the distinction made by Karl Brunner (1985, p. 214), "The classical approach to policy analysis thus understands policy not as a choice of specific actions but as a choice of general rules usually embedded in a set of institutions. We may juxtapose it under the circumstance as an 'institutional policy' to 'specific action policies.' " One level of private forecasting attempts to ascertain the effects on incentives of a change in a certain marginal tax rate, for example. Quite another level of analysis is involved in attempts to anticipate and project the implications of activist, discretionary, counter-cyclical stabilization policies.

Economic Policy Discretion

In addressing the subject of "The Limits of Economic Policy," Brunner discussed the sources of "Uncertainty in the Private Sector." Rather than beginning with an assumption that policy activism by governmental authorities is necessarily positive, as some writers do, or that policy is benign, as others have concluded, Brunner (1985, p. 223) simply asserts, "Government interventions confront members of society with a challenge. [Policy actions] modify private opportunity sets and thus induce various responses. Many of these responses address *an imaginative search to lower the impact of government intervention*" (emphasis added).

Brunner (1985, p. 223) continues, "Every intervention tends to produce unintended and unexpected consequences which eventually

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affect the evaluation and usefulness of the intervention." What makes private-sector forecasting of the effects of government actions so difficult, says Brunner (1985, p. 223), is that

> the social process governed by spontaneous interaction between agents in market economies operates as a vast system continuously creating and disseminating new information. Acquisition and interpretation of these flows of new information modifies private opportunity sets and expectations of future conditions. These revisions induce pervasive adjustments in behavior.

One is reminded of the Heisenberg uncertainty principle in physics: "The accurate measurement of one of two related, observable quantities, as position and momentum or energy and time, produces uncertainties in the measurement of the other, such that the product of the uncertainties of both quantities is greater." In monetary economics a variation of this idea became known as Goodhart's Law: The targeting of any one monetary variable alters the relation between that variable and real economic activity so that it no longer is a reliable indicator of the thrust of monetary policy actions. That is, the relationship holds only if few people know it and if policymakers do not attempt to control it.

Forecasts by Government

An appropriate question regarding economic forecasts issued by governmental authorities is, "Why do they do it?" Several possible reasons can be offered for government agencies making and issuing forecasts: (1) They are better forecasters than private-sector forecasters; (2) they have access to better, more timely data; (3) they have economies of scale (that is, they can do it more cheaply, possibly because they have bigger and faster computers); (4) they are providing a public good; (5) they are informing the public about consequences of policies they intend to take (for example, it has been argued that the credibility of the Bundesbank is so great that when it issues its forecast for inflation, the information alters private-sector decisions).

In arguing "The Case Against Monetary Activism," Brunner (1981, pp. 22–23) notes that there is assumed to be

> a peculiar asymmetry in the distribution of knowledge and information. The public sector, represented in particular by the central bank, stands implicitly as the guardian of truth with full and precise knowledge about the state and structure of the economy. The policymakers thus have in their minds the equivalent of the one and only true massive econometric model of the economy. The private sector, on the other hand, is sunk in the passive stupor of abysmal ignorance. . . .

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> Closely associated with this asymmetric distribution of knowledge there is a second implicit strand in the argument. "Government" is not only the guardian of truth, it is also the fountain of wisdom and the representative of moral principles. This means that the agents manning the political institutions can always be relied upon to execute policies fully reflecting the moral requirements of the "common good" on the basis of their mysterious possession of full knowledge of all the relevant factors. The pervasive occurrence in various forms of these ideas . . . should not blind us to their falsehood as statements about the world in which we live. The people operating the political institutions possess no superior knowledge about the process to be influenced, and generally have a vested interest *against* critical examination of their accustomed and usually questionable views.

Government Budgets

To assemble a budget, especially for a five-year period as Congress requires, one must use projections of such macroeconomic variables as output growth, employment, inflation, and interest rates. Such economic assumptions are not forecasts comparable to those prepared in the private sector. Numbers used for these variables for the first and possibly the second year of a five-year horizon may be someone's best guess about what might actually happen, given certain assumptions. For the most part, however, such numbers are not representative of highly probable actual conditions.

Government economists are not free to assume the policy actions they advocate will *not* be adopted, nor are they free to assume the Federal Reserve or Congress will actually do something they do not want those bodies to do. Private-sector forecasters have no such constraints. While private forecasters differ in their assumptions about actions that may be taken, they are not obligated to assume that their preferred policies will be implemented, as are government forecasters. In fact, private forecasters are free to assume government will do something to make things worse, while the government's own forecasters must foresee only positive implications of their recommendations or actions.

According to Brunner (1981, p. 32), private forecasters know that

> activist policymaking based on a particular response structure induces variations in this structure which eventually force adjustments in the nature of policymaking. This process lowers the likelihood, even with an optimal starting point, that activist policymaking can substantially contribute to raising the observed level of performance. It may actually convert "stabilization policies" into a sequence of destabilizing moves.

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The Fatal Conceit of Macroeconomic Policy[1]

One advantage of a market economy is that it is informationally efficient (that is, a market will function well even if each individual knows only his or her own preferences and opportunities). Conversely, when the government controls an activity, much more information must be collected and centralized, but this information is expensive and sometimes not available. The information is necessarily aggregated and averaged; it accounts for variations among individuals in only the crudest ways. This fact places another limit on the ability of the government to achieve its goals.

A slightly different aspect of the information problem involves timing. Fine-tuning the economy—responding to every small fluctuation—is not feasible largely because information, even if accurate, ages quickly. The information needed is simply not available or is generally not accessible in time to be useful. Also, there are lags in the system that are of variable and unknown length. Such lags often mean that policies will be responding to conditions that no longer exist and may tend to exacerbate the conditions that they are aimed at alleviating.

Macroeconomic Stability

Whether economic policies aim at specific markets or are meant to have a broader impact, the central problems are the same. Still, I shall focus attention briefly on some policy issues that arise in the area of macroeconomic stabilization.

It is sometimes argued that a market system is subject to unacceptably large fluctuations in income. If this were true, it would have welfare implications. First, the level of incomes, averaged over the business cycle, might be lower than would be true if the growth path of the economy were more stable. Second, even if the average level of incomes were unaffected by fluctuations, economic agents are generally risk averse (that is, most agents prefer a certain stream of income to a fluctuating stream, given that the expected values are identical). For these reasons, government might have a role in providing stability. Still, for intervention to be more helpful than harmful, it must overcome the formidable information and decision-making limitations I cited earlier.

An alternative view is that a capitalist economy is inherently stable. According to this view, actions by the government frequently are the main destabilizing factors in the economy. If market processes

[1]This section draws on my paper, "The Economic Role of Government" (Jordan 1988).

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are inherently self-correcting, faulty interventions by government—however well intentioned—will reduce economic stability.

In 1969, Axel Leijonhufvud (1973, p. 28) argued:

> The central issue in macroeconomic theory is . . . the extent to which the economy, or at least its market sectors, may properly be regarded as a self-regulating system. In what respects does it, or does it not, behave in such fashion? How well, or badly, do its "automatic" mechanisms perform?

Leijonhufvud's questions apply equally well today.

Much earlier, F. A. Hayek (1945) and W. H. Hutt ([1939] 1977) forcefully argued that a private, market-oriented economy is inherently resilient and naturally gravitates toward full employment of its productive resources following any kind of shock that has temporarily depressed economic activity. These shocks to various sectors of the economy include wars, droughts, energy-price or other commodity-price changes, perverse government policies, and substantial exchange rate movements.

This view of the natural, self-correcting tendencies of the economy is diametrically opposite to the stagnation thesis, which raised concerns about the adequacy of aggregate demand in the absence of governmental actions to ensure a sufficient amount of total spending. The notion of a "demand failure" persists to the present time. Several generations of economists in the private sector, as well as in government and in academia, have argued that even an economy that relies primarily on private property and a market mechanism to allocate resources could stagnate at less than full employment of its productive resources. These economists urge that central government ought to pursue an activist policy to ensure that sufficient demand is fostered. Their proposals have been implemented in the past, but the results have not always been as desired.

Unconstrained Monetary Creation

Let me turn now to one area where there may be the strongest case for an activist government policy intended to provide greater stability in the economy: the conduct of monetary policy. Not very many of us are happy with the conduct of monetary policy in recent decades, and it might be worth thinking through what sort of rules of the game we would like to see in this area, given our economists' understanding of the policy dilemma.

In the 20th century we have seen two policy-related phenomena that were left unconstrained by the Constitution's framers. One is the unlimited power of the federal government, unlike state governments, to issue interest-bearing obligations to finance expenditures.

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The other is the unlimited discretion of the central bank (which did not even exist until 1914) to substitute its own non-interest-bearing obligations for the interest-bearing obligations of the U.S. Treasury.

Money performs several functions in an economy. It economizes on transactions costs and on information costs, since all persons accept the same money and are aware of its value. There also are arguments for a monopoly money supply, since the use of only one type of money in an economy will reduce information costs. But what sort of monetary policy is consistent with the spirit of the Constitution and yet responsive to income stabilization concerns?

The government (or, in the current age, the Federal Reserve) in its money-creating function must be careful not to exacerbate cyclical fluctuations. Milton Friedman and Anna Schwartz (1963) have argued that the length and severity of the Great Depression of the 1930s was caused primarily by inappropriate Federal Reserve policies. Today private-sector forecasters must consider the possibility that such major policy mistakes could be repeated.

The invention of open-market operations as a method of financing government—wherein non-interest-bearing obligations of the central bank are created to purchase (and thereby cancel) direct government obligations—has proven too tempting to resist in the 20th century. In an era of large fiscal deficits, resorting to the monetary printing presses as a lesser of evils to cutting real spending or raising explicit taxes will be a temptation to some politicians. The "dismal arithmetic of monetarism," noted by Sargent and Wallace (1981), warns portfolio managers that under some conditions debasing the purchasing power of money becomes unavoidable. Ignoring such analyses and failing to monitor government behavior would prove very costly.

Economists and Policy Advocacy

Many contemporary politicians and economists do not seem concerned about the enormous power of the central bank. The inclination of most politicians toward an institution that seemingly augments the power of the political authority to tax and spend is not surprising. More puzzling, at least to me, is that economists rarely challenge the premises of unbridled central bank authority. In fact, economists often seem to be making a case for still greater scope of centralized authority.

We all know that the jargon of free markets has broad appeal, even to those who have a very different agenda. We are familiar with business leaders who talk about free markets, then seek protection from imports or defend regulatory barriers to competition. We also

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are not surprised by the politicians who talk about free markets and private enterprise, then vote for all sorts of governmental intrusion in the economy.

To paraphrase Winston Churchill's statement about democracy, "Capitalism may not be a perfect economic system, but it is much better than all the alternatives." Nevertheless, we see many of our fellow economists advocate governmental policies in ways that suggest either they do not believe that private is better, or they do not like the outcome of a nongovernmental approach. If economists do not consistently profess the virtues and superiority of a market economy, who will? While there are many dimensions to this phenomenon, I shall comment only on the rhetoric of the macropolicy debate.

Many of us claim to believe in the inherent resiliency of a market economy, but confidence in the magic of the marketplace differs among us. Do you accept that there is an unseen hand that keeps the economy expanding? Are you willing to argue that government attempts at fine-tuning the economy are more likely to do harm than good? Or do you look to Washington to do something when a firm, a sector, or a region confronts a serious economic challenge or threat?

Doing something to deal with this or that economic ill typically consists of actions that result in increasing government spending—including so-called jobs programs—or cutting personal taxes to increase personal disposable income, or increasing credit availability, or attempting to reduce market interest rates, or undertaking to influence exchange rates. In the late 1980s, the return to demand management took the form of pressure on the economic policymakers of Japan and Germany to engage in pump-priming stimulus to their domestic demand. Even economic spokesmen in the Reagan administration called for demand stimulus policies by other countries.

My concern is that endorsement or advocacy by economists of activist policies communicates considerable lack of faith in market mechanisms. On a daily basis we encounter economic arguments for intervening in foreign exchange markets, tightening or easing monetary policies, or changing government spending or tax policies for the express purpose of augmenting or diminishing aggregate demand. In each case the argument suggests that markets cannot be relied on. If our profession does not have confidence in the self-correcting tendencies of a market economy, then it is unlikely that business leaders or politicians are going to be willing to rely on the inherent resiliency of a capitalist economy.

Conclusion

The U.S. model of a capitalist economy is far from perfect, but it still provides a better environment for upward mobility of its

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participants and a greater aggregate standard of living than any known alternative. Where there are shortcomings, the flaw is more likely to be too much intrusion of government at some level, rather than too little government involvement.

During the 1980s, we witnessed less government regulation of domestic industries than in the previous two decades, but more political sentiment for governmental intervention in international trade. We saw the Reagan administration in its first term extol the virtues of the magic of the marketplace, then in its second term advocate old-fashioned pump priming of aggregate demand to speed up world expansion. In the first term, the Reagan administration prohibited discretionary intervention in foreign exchange markets; in the second term, Reagan policymakers were advocates of a policy of deliberately reducing the international (and domestic) value of the currency.

At the beginning of the past decade, Congress seemed determined to impose some constraints on the discretion of our central bank and to force the monetary authorities to adhere to some rules. But that determination did not last long, and by the middle of the decade we had returned to the purely judgmental approach to monetary policy that prevailed during the 1950s and 1960s.

Today, although the media have been slow to catch on, the old, false dichotomy between pro-growth and anti-inflation policymakers has been substantially diminished. We still have considerable activism and attempts at fine-tuning the macroeconomy, yet there also seems to be a healthy degree of humility about what can be done by government to "do good" rather than make things worse.

Nevertheless, individuals elected or appointed to government office will inevitably bring to their position a zeal to "find out what's wrong and fix it." Given that, the rest of us have no choice but to attempt to anticipate, to cope, and to survive, which means we must forecast. To fail to do so could be fatal.

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The Case for Market-Based Forecasting

Lawrence A. Kudlow

My comments will begin with some of the themes that Jerry Jordan mentioned in his paper, especially the theme of government policy rules, which is, of course, crucial to Karl Brunner's work. Brunner was a great friend and teacher of mine, as is Jerry Jordan. So there is a little bit of a historical conspiracy. But the question of rules in government policy is irrevocably tied up in a very general point: the use or the utility of the markets and market prices, which is really the thrust of my comments.

I agree with the people who have mentioned how poor economic forecasts are. Markets strike me as really the best—the only reliable—forecasting tool. In the same respect, I think markets are the only reliable policy tool. And as Jordan noted in his distinction between government forecasting and private forecasting: Private forecasting is largely defensive in response to government events, which are frequently unpredictable and irrational.

Markets, Information, and Policy Formation

Inside government, the use of markets—not only to discipline policymakers but to provide information—can be an awesome tool in helping policymakers chart the right course. When I served in the Reagan administration, I thought we made a much greater use of markets than others had— not only in the sense of letting markets tell us what was right and wrong, but also in the sense of deregulating them to increase their efficiency. To the extent that we enjoyed a much better economic success in the 1980s than was the case in the 1970s—not perfect but substantially better—I think my point of view is vindicated.

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On the margin, the last announced piece of information is always going to be in an open market, and free markets will always know more than the most complex and comprehensive econometric model or the 500 some odd Ph.D.s at the Federal Reserve Board.

Why Forecasts Fail

One of the reasons econometric models do not work, never have worked, and, in my view, never will work, is that economics itself is simply not a Newtonian physical science. Professionals, academics, and policymakers have tried to make it into a physical science for the past 40 or 50 years, particularly since the rise of Keynesianism and the decline of classical thinking. But the physical science approach does not work. So if we are now having a general rebirth of classical thinking and the classical approach—because of the work of Friedrich Hayek, Milton Friedman, Karl Brunner, and many others—then we have to admit that economics is really not a physical science. It is not clockwork. Time never stands still.

We are dealing with human behavior, which is always changing, not mechanical constructs. And human behavior is always evolving and changing in response to events, both government and nongovernment. We have a historical continuum here. Because human behavior is always evolving, I believe that the evolution of economics and human behavior is a lot closer to biology than it is to physics. And it is a lot closer to Darwinian evolution theories because, after all, individuals compete, which was also the essence of the beginning of biology.[1]

Markets as Forecasting Devices

My experience in government and the private sector has convinced me that markets are crucial for policymaking, for rule making, and for forecasting. Indeed, I now rely almost exclusively on using markets as forecasting devices. In forecasting inflation, for example, I pay little attention to the CPI, or to data from the Commerce Department, the Treasury Department, or the Federal Reserve. I certainly examine the data, and my clients expect me to comment on it; but I do not believe any of it. Gold and commodity prices tell me a much better story about inflation, because they are market prices.

Monetary Policy

With respect to monetary policy, I believe more and more strongly that market prices have more information than monetary aggregates.

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In particular, I have argued, written, and defended the view that gold prices and, to a lesser extent, the equally important sensitive commodity indexes, are the only way to capture the mix or the balance between the money supplied by the Fed and the money demanded by the private economy. It is a question of the interaction of money supply and money demand. We cannot focus on only the supply of money, because, after all, the ultimate game is to get inflation as close to zero as possible. I believe we can achieve zero inflation by raising the domestic purchasing power, or value, of our money, which is largely a function of the demand for money. I do not mean the demand for credit; I am not talking about commercial loans. I mean the willingness of people to hold financial balances or financial assets such as stocks and bonds. If people are willing to hold those monetary balances, then the price or value of money goes up. Hence, money buys more goods and services, and inflation goes down. In my judgment, the price of gold is the ultimate indicator of the purchasing power or value of money. The market value of gold tells us whether the demand for money is in line with the supply of money created by the Federal Reserve.

I am not dismissing the argument to use the quantity of money as an indicator of monetary policy, but I think using monetary aggregates is a tricky business. Even the most rigorous and consistent students of money are still confused about which monetary aggregate—the base, M1, M2, M3—is the right indicator to gauge monetary policy.

In choosing a measure of the quantity of money, there is a tendency to select the one that tells the story you want to tell rather than the one that is actually the "correct one," which is almost unknowable. We have all played the game—whether it is M2; whether it ought to be the monetary base; whether it ought to be the unadjusted base or Federal Reserve credit, which comes off the balance sheet of the Fed; or whether it should be bank reserves or other things. For example, if the growth of M2 is below the Fed's target range while the monetary base is exploding, which is the right aggregate? If I distrust the monetary base because currency is flowing abroad or because I know the Fed has reduced reserve requirements on some of the institutional deposit accounts, and the so-called reserve adjustment formula might not yet have gotten it right, then I do not want to use the base as an indicator of monetary policy. Instead, I might look at the Fed's balance sheet and use adjusted or unadjusted Federal Reserve credit. But even that indicator may not be correct. The domestic banking system may not be using the Fed's high-powered credit.

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No one really knows which quantity indicator is correct. My answer is, "Why don't you just look at the price of gold or commodity indexes?" Although no Fed governor has said it and although the Fed chairman has never explicitly discussed it, I think we are actually operating on a de facto, Bretton Woods–style, *domestic price rule* whereby the value of the domestic dollar is linked to gold and whereby the supply of dollars created by the Fed is basically linked to a yardstick called gold. When the Fed operated in this Bretton Woods fashion in 1989 and 1990, the money supply was, on average, well controlled. M2 growth was about 4 percent, nominal GNP growth was about 5 percent, and gold stayed, on average, at around $375–$380. We had a terrific bond rally; long-term interest rates, which can stimulate corporate investment and housing, dropped from 9.5 percent to 8.5 percent in 1990. Short-term rates went down even more. There was virtually no inflation risk in the market. The Fed was operating exactly the right way.

When the Fed went off the Bretton Woods price rule in the middle 1980s and decided to increase the money supply, we paid heavily for it. Gold and interest rates skyrocketed, which led to the stock market crash of 1987. That crash was, among other factors, the precursor of the recession. Therefore, the stock market crash of 1987 turned out to be a better forecaster than some people thought.

If the Fed stays with the implicit price rule it is now using, we are going to be in better shape than we think. I like the price-rule approach because I think it captures not only the supply of money in the Fed's actions but also fiscal policy. Taxes and regulations count a lot in monetary value and in the inflation outlook. Taxing, spending, and regulating can affect inflation. If we increase taxes, if we raise government spending as a share of GNP, or if we regulate more heavily, we will reduce incentives, efficiency, and productivity in the economy (that is, we will reduce real economic growth).

If money growth is constant, but you shrink or reduce the growth of goods and services, you cause inflation from the supply side because you roll back incentives. Markets will respond. Look at the behavior of the Dow Jones in 1990. It rose to 3,000 in the late winter and early spring. At almost the moment President Bush moved his lips, the Dow's rally ended. As the shape of that budget deal, which was nothing but a high tax and spending package, became clearer and clearer to the markets—before Saddam Hussein and after Saddam Hussein—the Dow Jones dropped about 700 points. And the market was headed lower, until, in my judgment, (1) the Fed made it clear it was not going to monetize the oil price shock but was going to stay with the gold-price, Bretton Woods–type rule; and (2) it became

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obvious that the Bush administration was going to move back on the supply-side growth path. The combination of a steady, domestic Fed price rule and what looked like the return to growth economics moved the Dow from 2,300 in the fall of 1990 to 2,900 and higher by early 1991.

I am not attacking monetarism, which is, after all, the study of money. The Federal Reserve controls our money supply, however it is defined. But there is good monetarism and bad monetarism. Bad monetarism says we are going to fine-tune the monetary aggregates or some measure of the quantity of money to hit some artificial target. Bad monetarism also believes that the Fed can fine-tune nominal GDP growth over short time intervals. Some very bright people of great integrity whom I respect are now saying we should significantly increase the growth of M2. Such a policy, however, would undermine the price rule, drive the price of gold and interest rates—particularly long rates—sky high, and wreck whatever chances of recovery we have.

In contrast, good monetarism says the Fed should add or withdraw reserves while keeping a sharp eye on gold and commodities as the value-of-money yardstick. That kind of monetarism is really what the Fed is now engaged in and is the right way to go.

Conclusion

I want to end on an optimistic note, because of my dislike for the stagnation model. Why does everyone always assume the worst case? If the Fed continues with a domestic price rule, then interest rates of 3 to 4 percent for Treasury bills and 5 to 6 percent for Treasury bonds should result. The budget deficit would then be eliminated, because over the next five or six years we would save a couple of hundred billion dollars in net interest expense compared to the current services baseline. I think a global case can be made that countries are operating on domestic price rules everywhere, thereby punishing inflation. That is why we have have a big bull market in bonds and stocks in almost every national currency, which, in turn, should move the exchange rates back into equilibrium.

Markets are made up of the decisions of millions of people who want prosperity, progress, and better living standards. When governments interfere, markets will punish right away and get policymakers back on the right track. The Malthusian notion, which is the stagnation view or the global capital-shortage view, can be rejected and discarded as long as we let markets operate freely and pay attention to them as private citizens and government policymakers. Population

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growth is a plus because of the potential creativity of individual men and women. Resources are not scarce; they are limitless—because technology is boundless. If we stay close to the free market, the 1990s can be another decade of strong growth.

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My Experience With Government Forecasting

Paul Craig Roberts

When Jim Dorn asked me to briefly relate my experience with government forecasting, he thought that perhaps my background might cast some light on the question of whether better models and active policy management can produce a more successful economy than clear-cut rules in harmony with normal incentives.

I must confess that I am amused with the notion of myself as a forecaster. Nevertheless, I have had two government jobs that forced me to deal with the world of forecasting. In 1976, I was the Republican party's economist on the House Budget Committee. Because the committee had only one economist, that qualified me for the grand title of chief economist. And in 1981 I was assistant secretary of the Treasury for economic policy where I learned that sometimes when you get in a job that is not your specialty, you learn more about it than anybody ever wanted you to know.

The CBO Spending Bias

The Budget Control Act had been passed in 1974, and in 1975 there was a dry run on how the new budget process would work. The new process began in earnest in 1976 and led the Republicans to conclude that they needed an economist. The expectations the Republicans had for the budget process bore no relation to reality. They thought they had pulled a coup on the Democrats, because they had gotten the Democrats to vote for the act. The Republicans were convinced that this act meant no more deficits; their theory was that the Democrats caused deficits as they voted on a lot of individual spending bills that allowed them to overspend the revenues on a piecemeal basis. Republicans thought Democrats would

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lack the courage to vote for a deficit if they actually had to vote for one in a budget resolution.

Of course, the process did not work that way. What it did was to institutionalize Keynesian fiscal theory. The first budget resolution that came forward was really a choice of deficits. There was a moderate deficit path, which was the path policymakers had decided they wanted. The moderate deficit path produced the appropriate Phillips curve tradeoffs between employment and inflation. Then, to show why Congress should choose this moderate path, the Congressional Budget Office (CBO) would bracket the moderate deficit with a low deficit and a high deficit. The low deficit had low inflation, but it had an unacceptable unemployment rate. The high deficit showed a booming economy and good employment, but inflation was too high. The Republicans were quite shocked when they discovered that, if they rejected the moderate deficit, they were faced with arguing in favor of a small deficit with unacceptable unemployment or a big deficit with unacceptable inflation.

The process was more or less rigged in favor of spending because, having determined that Congress must choose a deficit, policymakers wanted to be sure that members of Congress chose the right way. Multiplier rankings from the CBO's multipliers' project showed that government purchases and public service employment programs were much more effective stimulants to the economy than a reduction in the tax rate. Indeed, CBO reports and testimony said that cutting income tax rates was an inefficient way to stimulate the economy. Society would not get much bang for the buck. Moreover, the CBO produced simulations showing that cutting corporate taxes would reduce investment. After various model runs showed that cutting corporate taxes would reduce investment and cause the GNP to fall, I began investigating what kind of an economy that was. I discovered it was one in which investment was not sensitive to better after-tax earnings, but was very sensitive to the interest rate. If corporate taxes were cut, the deficit went up. If the deficit went up, the interest rate went up. If the interest rate went up, the investment rate went down, and GNP fell.

Finally, to make sure there was no alternative to the expansion of government spending (the only question was how much each year it would expand), policymakers argued that incentives simply could not work because a tax cut would let people meet their income and saving targets with less work. In other words, if Congress cut taxes, people would increase their leisure. I used to ask policymakers how taxpayers could maintain their existing levels of income if, in the

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aggregate, they responded to a tax cut by working less. This question was a long time catching on.

That was the nature of my experience with the forecasting models used to actively manage U.S. economic policy during the 1970s. The experience provides an understanding of why the supply-siders were able to bring the Keynesian models into question.

Soon after this experience, Karl Brunner asked David Meiselman and me to write an assessment of the CBO and its approach to macroeconomic policy for one of the Carnegie-Rochester Public Policy Conferences. David and I wrote:

> [I]n the CBO analysis there is demand without supply, inflation without money, interest rates without capital, output without inputs, employment without wage rates or a labor market, and investment without saving or any change in capital stock. Expectations are assumed to be static and consumption is assumed to depend only on current disposable income. Fiscal policy is seen as affecting aggregate demand with no incentive or disincentive effects on supply [Meiselman and Roberts 1979, p. 283].

The Troika Process of Forecasting

Things began to change in May 1980. One of my principal projects as an aide in the Senate (where I moved after the House Budget Committee) was to get hearings on econometric models. This was a multiyear effort, because the CBO did not want any hearings on the nature of the models. However, the Democrats reached the point where they were upset with stagflation and the fact that conventional wisdom had no answer as to how to end it. They did not like the Phillips curve tradeoffs, and politically stagflation was very disappointing. Democratic committee chairmen, such as Lloyd Bentsen who was chairman of the Joint Economic Committee and Russell Long who chaired the Finance Committee, started changing the tune. As soon as they changed, the forecasting community changed its tune. Hearings began in May 1980. People who had been fighting the supply-siders now claimed to have supply-side models. At the hearings, Otto Eckstein and other econometric forecasters explained the importance of supply-side economics and why it was in their models.

But this change of attitude did not carry forward into the executive branch. Forecasting was based on the troika process. First, the Treasury, Office of Management and Budget, and Council of Economic Advisers (CEA) staffs would make a forecast. It would come up to my level and then go to the secretary of the Treasury, the budget director, and the CEA chairman. I was struck by the power of tradi-

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tion in forecasting. We were supposed to be a supply-side regime, but still the language was Keynesian demand management. The Treasury never produced anything but a static revenue forecast. The Reagan administration received a bum rap for making a Laffer curve forecast that the tax cuts would pay for themselves. Actually, the administration predicted that the tax cuts would lose every dollar of revenue, and that was the only forecast you could get out of Treasury. The Phillips curve won over the Laffer curve, and the administration overestimated the inflation rate. That overestimation caused revenues to be overpredicted. In the first drafts of our economic program, the supply-side policy was described in Keynesian demand management terms, and there was little anyone could do about it.

Ending the Status Quo

A summary of my experience in public policy leads me to conclude that two chief factors drive that policy: One is to expand the government's power, and the other is tradition. Even if the first driving force is stalemated, tradition itself can carry on.

We all know about the problems of public choice that James Buchanan, Ronald Coase, and Robert Lucas have raised. There is also the problem of information. When I was in graduate school at Berkeley, some professors tried to get us to study econometrics. But one professor had us read Oscar Morganstern's book, *On the Accuracy of Economic Observation,* which was published in the 1950s. That book made it difficult for us to study econometrics, because Morganstern showed that the data were so bad you could not get valid econometric results. The data seemed to range from bad to fabricated. He related an experience with a Third World official who said that since the United States was so insistent on statistics as a basis of foreign aid, his country would simply produce whatever statistics were necessary to get the aid.

Scientists discount intuition and stress the power of empirical facts, but, in their own discoveries, they rely on instinct. Empiricism led us to believe that we could improve on the invisible hand if only governments had the power to order economic affairs. I think it is time to try a few rules that are well attuned to normal human incentives, and to get as far away as we can from the status quo of active policy management.

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Has Macro-Forecasting Failed?

Victor Zarnowitz

What to Ask, Why, and How?

The title of my paper, "Has Macro-Forecasting Failed?" serves a good purpose, even though it seems to be somewhat provocatively phrased. We are reminded that claims to predict the future must be properly modest or they will prove disappointing. The more that forecasts matter and the more people that depend on them, the greater the dangers of having overstated promises and unrealistically high expectations—and macroeconomic forecasts matter greatly when used as guides by public and private decisionmakers.

The question deserves a straightforward, but careful, answer. A simple "yes" or "no" lacks meaning. Forecasting the economy's course, even short-term and in the broadest outline, is a mixture of art and science that can be very imperfect and sophisticated at the same time. Thus, we face a problem whose solution depends on the treatment of more fundamental questions about (1) what the forecasts are and why they are needed, and (2) what we can reasonably expect of them.

It is easy to think of needs, uses, and standards associated with macro-forecasting that will readily show it as a failing enterprise. What is more difficult but also more important is to decide which legitimate and credible applications of forecasting would, in principle, allow our title question to be interesting (i.e., capable of being answered either positively or negatively according to some sensible and, so far as possible, quantifiable criteria).

To establish what forecasters can and should do, we must study the record and assess the probable future of their endeavors. I can sum up these large subjects only selectively in this paper. Thus, I

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shall concentrate on what I studied firsthand, namely annual and multiperiod quarterly forecasts of the principal aggregate variables in the United States during the post–World War II era. The predictions refer to levels and changes of national output, employment and unemployment, consumption and investment, prices, and interest rates. They cover short and intermediate horizons of one or two years along with one to four, or at most eight, quarters ahead.

Movements associated with the business cycles prevail over such time spans. Irregular variations from random causes and intrayear, approximately periodic, seasonal movements influence heavily most economic time series in the shortest run measured in weeks and months. But the forecasts are generally in quarterly and annual units, and they aim at seasonally adjusted values wherever seasonal movements exist. Furthermore, the random noise in the series is unforecastable. Hence, the systematic part of the time series covered by the macro-forecasts consists mainly of cyclical movements and, to a lesser extent, elements of longer trends. It follows that a forecast should be judged successful if it approximates reasonably well that part of its target. The trend-cycle movements include the effects of past shocks and seasonal innovations that may or may not be knowable, but those movements presumably have important endogenous ingredients as well.

The task of forecasting is more difficult than the term "systematic" may suggest. Business cycles are persistent and recurrent, but they are by no means predetermined or periodic. They tend to be pervasive but affect different variables and sectors in different ways. Fluctuations and long trends in real growth and inflation interact with each other and contain stochastic elements. The economy in motion is a complex of dynamic processes, subject not only to a variety of disturbances but also to gradual and discrete changes in structure, institutions, and policy regimes. No wonder there are few, if any, constant quantitative rules (e.g., time-invariant linear econometric equations) to help the macro-forecaster effectively and consistently over more than a few years or from one business cycle to another.

Indeed, in social sciences and human affairs generally, it is only prudent to recognize from the outset that the future simply cannot be foreknown. Any maker or user of economic forecasts, therefore, must always be prepared to be wrong, even if the errors are, at best, relatively small and unpredictable. However, this probability of error does not alter the fact that most decisions that matter are inevitably forward-looking and, hence, involve forecasts. Where macroeconomic forecasting cannot be avoided, it is probably advisable to make those forecasts explicit and as good as possible, given the available

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cost-efficient methods and information. Forecasts are needed and can be useful even when imprecise.

My basic approach, therefore, is to ask the following of the available data: What professional standards have economists who are engaged in macro-forecasting been able to attain and maintain in competing with each other and alternative methods? We have learned much in recent times by assembling and examining measures of absolute and relative accuracy for reasonably representative samples of macroeconomic forecasts (my own published work in this area goes back to 1967). By now we would expect most professional predictions to be, on average and over time, much better than naive mechanical projections, but that expectation is a minimal requirement: To be successful, predictions should exceed significantly the more sophisticated univariate and multivariate time-series models. Further, it is desirable that the forecasts be free of such systematic errors as could have been either prevented by good modeling or eliminated by learning from the past. The extent of such biases depends on the stability of economic processes, the lags and costs of adjustments to unanticipated change, and the relative contributions to the forecasts made by models and techniques on the one hand and by new informational and judgmental inputs on the other.

In principle, what matters most about macroeconomic forecasts is their usefulness for decisionmakers both in government and in the private sector. True, this criterion is most difficult to apply directly as little is known generally about the loss functions of users and the effective costs and returns to them associated with such forecasts. But even here not all is lost; we can safely assume that a high positive correlation exists between the usefulness of forecasts and their measurable quality attributes, notably high relative accuracy.

Forecasting as a Competitive Industry: Why No One Is Best

Peering into the future is an ancient occupation frequently characterized by great hazards and by corresponding vagueness or obfuscation. Economic prediction as an artful pursuit or a game of chance undoubtedly has a long past, but authentic forecasting of well-specified future values of aggregative variables is of recent origin. A responsible appraisal requires a recorded history of forecasts that are not only explicit and verifiable but also sufficiently numerous and consistent. This requirement rules out vast amounts of data, both old and new, and urges concentration on the longest available time series of reasonably comparable predictions from reputable sources.

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Economic and financial forecasting in the United States today is an industry of significant size. Many forecasters belong to the National Association of Business Economists, whose membership numbered more than 3,300 in 1990 (it numbered 322 in 1959, the year NABE was founded). The forecasting units vary from individuals and small teams to sizable divisions of some large corporations and multibranch specialized consulting firms. Some of the latter operate large-scale econometric models and provide customer services internationally. Business demand for forecasts of numerous more or less aggregative variables is largely satisfied by subscriptions to such services. For small numbers of primary macrovariables, special publications survey groups of professional forecasters monthly or quarterly. Thus, the U.S. market for these forecasts can probably be described as a mixture of competitive and oligopolistic elements, with the overall number of sellers relatively large and the barriers to entry low.[1] In addition, some macro-forecasts are provided by government agencies, essentially as public goods, and some can be acquired at very low cost from the press. U.S. government forecasts are designed to serve as inputs into economic policymaking, and they originate in several agencies. Some forecasts are publicized but most are for internal uses only.

Forecasters compete, adapt to continuous change and new developments in the economy, and try to improve and differentiate their products. Few leave their models and techniques unchanged for long. Moreover, success in forecasting may be occasional and fortuitous or intuitive. Hence, a particular forecaster's record may not be reliable as a basis for inferences on how he or she will perform in the future. The shorter that record, the more uncertain are such inferences. Ranking the forecasters on how well they predicted changes in a single short period is quite risky and not very informative; in the next period the ranks are very likely to differ considerably. Nevertheless, such comparisons are commonly made at least once each year in the business press.

That there is great inerest in finding out "Who forecasts best?" is certainly not surprising, and many recent studies address this question. However, their well-established general conclusion is that no one forecaster does or, equivalently, that many forecasters do. That is, the measures of overall accuracy surveyed (typically, mean absolute errors [MAEs] or root mean square errors [RMSEs]) do not show

[1]In other highly developed countries, macroeconomic forecasting is generally more concentrated in a few private sources or in government agencies and publicly supported research organizations.

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any of the compared individuals or organizations to be consistently and generally superior to others. The rankings vary for different periods, variables, and horizons covered; they also depend on the criteria and measures applied. Moreover, the differences between the MAEs or RMSEs across the best-known sources are mostly small and of uncertain statistical significance (which cannot be directly tested since the forecasts are not independent).[2]

There are good probable reasons why the principal macro-forecasters cannot be ranked unambiguously by any standards of accuracy. Authors of predictions that are matched by variable, time of issue, and target period draw on much the same body of data; often use similar methods; are exposed to common current events and attitudes; and, to some extent, interact and influence each other directly. A free market exists in economic data and ideas, and advances in forecasting technology are soon open to all practitioners. This openness tends to reduce the diversity of individual predictions created by the undoubted fact that forecasters differ greatly in theoretical orientation and training and in talent and experience. It also presumably keeps the comparative advantages of the initially better-endowed forecasters more temporary and smaller than they would otherwise be.

Successful forecasting has diverse ingredients that are unlikely to be monopolized as a combination. Some functions, like the exploitation of time-series properties in the data, are best performed by the computer and its programs. Other functions, like the quick and efficient sifting of new information, would seem to require much specialized skill and experience. The ability to develop superior judgmental forecasts on this basis is probably a rare individual gift that cannot be easily taught, transferred to others, or applied on a large scale.

All of this helps to explain why concurrent matched predictions from different sources show both common trends and much dispersion around them. The frequently alleged predominance of a single

[2]For example, of the six sets of forecasts examined in Zarnowitz (1979), each was "best" for at least one variable, subperiod, and span considered. The comparative advantages were generally quite scattered, however, except that forecasts released later in a quarter, being based on more information, tended to be more accurate than those made earlier in the quarter. (This factor can be isolated by comparing early and late predictions from sources that forecast monthly or twice per quarter.) Similar results are reported elsewhere (e.g., in McNees 1979 and, for the United Kingdom, in Wallis 1989).

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"consensus forecast" is mostly a myth.[3] The above arguments are also consistent with the observation that the interforecast differences are not persistent enough to give rise to systematic ratings of predictive performance.

The Multiplicity of Methods and Models

The coexistence of many different forecasts aiming at the same targets and continuing to have significantly dispersed errors is in itself an indication that no single model or technique is generally expected to prove consistently superior to others. If any such winner were believed to exist, it would soon come to enjoy the first preference of the profession and be universally adapted. Instead, the market has room for a sizable and diversified activity of macroforecasting.

Indeed, substantial and uncontroverted evidence from surveys of professional forecasters shows that they distinctly favor several approaches in varying combinations. For many years, questionnaires of the quarterly *Economic Outlook Survey*, jointly conducted by the American Statistical Association (ASA) and the National Bureau of Economic Research (NBER) during 1968–90, collected information on the premises and procedures incorporated in the members' forecasts. More than 70 percent reported using—and from half to two-thirds ranked first—the "informal GNP model" in which the major expenditure components of GNP are predicted in various ways, combined into an overall forecast, and then checked and adjusted for plausibility and internal consistency. This model is itself a mix of procedures applied eclectically and flexibly with large elements of judgment, not a well-defined method. Leading indicators were ranked second by most respondents and were used by large numbers. Somewhat smaller proportions of the survey membership mentioned anticipations surveys, which generally ranked lower. The percentage of users of outside econometric models rose from less to more than half between the late 1960s and early 1980s. About one-fourth of the respondents had their own econometric models, and the proportion of those ranking such models first was similar. Finally, other methods such as time-series models were specified by fewer than 20 percent

[3]A forecaster must indeed expect his results to be compared with that standard that is widely reported by professional associations, business magazines, and other media. But there are as many average forecasts as there are groups surveyed, and they may at times express little agreement. The averages are not well specified, and they lag somewhat behind the release of many noteworthy forecasts for the same period.

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of the ASA/NBER survey participants and were preferred by about half of those respondents.

Different methods tend to complement each other (e.g., new readings on monthly cyclical indicators and the latest results from an investment or consumer expectations survey may modify forecasts from econometric models or the informal approach). This is the presumed reason why the dominant forecasting practice is to use various combinations of these techniques. Other sources confirm this important lesson.[4]

The forecasters' methodological choices, as reported in the ASA/NBER surveys, do not appear to be associated with significant differences in predictive accuracy. Those who preferred econometric models in their own work were not as a group systematically better than those who preferred the informal approach. However, according to comparisons based on first rank only, subscribers to outside models—a subset dominated by large companies using well-known econometric service bureaus and their own professional staffs—had a marginal advantage over the other categories. On the whole, these test results are consistent with the view that combining different procedures helps, particularly when done by experienced forecasters (Su and Su 1975; Zarnowitz 1971, 1984).

The 1950s and 1960s witnessed a great ascendancy of macroeconometric forecasting based largely on models of Keynesian provenance and the paradigm of "neoclassical synthesis." The prevailing belief was that business cycles could be subdued by fine-tuning fiscal and monetary policy, aided by sufficiently early and accurate predictions. The naively overconfident nature of such views became increasingly clear as forecasting and policy failures multiplied during the late 1960s and 1970s. Inflation and unemployment both trended upward, which contradicted the idea of a stable Phillips-curve tradeoff. New shocks to oil and other major input prices caused adverse shifts in aggregate supply, which undermined the effectiveness of aggregate demand management. Discretionary fiscal policies have long proved too sluggish and inflexible to be effectively countercyclical.

[4]Thus, the annual surveys of NABE in 1975–79 show that 52–60 percent of their members preferred "eclectic judgmental" and 22–28 percent "eclectic econometric" methods. A special mail survey sent to the Blue Chip forecasters in 1987 resulted in the following mix of average contributions: judgment, 48 percent; econometric model, 28 percent; and time-series analysis, 24 percent. Even the organizations with their own large-scale econometric models (e.g., BEA, Chase, DRI, Kent, UCLA, Wharton) assigned sizable weights to judgment (20–50 percent, on average 30 percent) and other elements such as time-series methods, current data analysis, and interaction with others (10–20 percent). See McNees (1981).

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But discretionary monetary policies, though more potent, were frequently not any more timely and successful. This lack of success was not always or necessarily due to lacking foresight; often the fault lay in wrong indicators and targets, divided opinion and indecisiveness of authorities, or miscalculations of long and variable lag effects. The point is that monetarist models, too, failed to produce dependable forecasts for the conduct of macro-stabilization policies. The crudely monetarist-oriented Fed tactics that temporarily replaced the Keynesian regime in the late 1970s succeeded in finally eliminating the unbearably high and volatile inflation and interest rates, but only at much higher costs than expected and by contributing heavily to the two recessions of 1980–82.

Criticism of both Keynesian and monetarist model specifications and predictions naturally led to new developments, some of which were much needed and promising: a resurgence of theoretical and empirical studies on business cycles, on the formation and role of expectations, and on the microfoundations of aggregate supply and price adjustment. New theories emerged, stressing rational expectations, imperfect information, real business cycles, wage and price contracts, and rigidities. But the new ideas and methods are still in the stages of development and academic debate: They have not yet given rise to econometric models and demonstrated their usefulness for macro-forecasting.

People who must predict changes in the economy at frequent and regular intervals are typically absorbed by the technical requirements of monitoring and processing information, analyzing current developments, and preparing interpretive reports. Most are pragmatic in using any data and approaches deemed helpful; few spend much time on working with specific theoretical models. The task of testing various hypotheses, models, and methods is left largely to the academic economists. A very recent study of forecasters who were cross-classified according to their theoretical preferences as well as methodological choices concluded that "No one ideology or technique yields consistently more accurate forecasts than others."[5]

[5]See Batchelor and Dua (1990, p. 3). They used the Blue Chip 1987 survey mentioned in note 4 above and examined annual forecasts of real growth, inflation, and interest rates made in 1976–86 by 44 respondents. The weights placed on the listed theories were as follows: Keynesian 43 percent, monetarist 20 percent, supply side 12 percent, RE 8 percent, Austrian 4 percent, and other 13 percent. Some support was found for the inference that the Keynesian-econometric combination had an advantage, but this could reflect the fact that the modern versions of other theories and methods developed later and so had adherents with less practical and diverse experience. The forecasters in the sample generally used elements of more than one theory and relied on more than one technique.

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How Accurate Have the Forecasts Been?

Progress in forecasting, as distinguished from occasional successes that may be due to chance, can come only from advances of science in discovering and quantifying important regularities. Although such predictive relationships are found in macroeconomics, their range and duration are probably more limited than they are widely believed to be. The economy grows and fluctuates in various ways, reflecting the diversity of human behavior that causes and reacts to the omnipresent change. Of necessity, economic theory simplifies starkly the motivations of individual and collective action, thereby attempting to reduce the uncertainty surrounding economic change. In the process, economists risk taking their models too seriously and overestimating their ability to predict the movements of the real economy. As already noted, some did succumb to this error in the recent past when the economy enjoyed relatively stable growth and rising prosperity, while macroeconomics also appeared to be doing well. Under such conditions, the informed lay opinion was only too willing to accept the optimistic claims of the experts. The present danger is one of overreaction in the opposite direction.

Actually, the true macro-forecasts from credible sources were never as good as many had once believed; nor were they as bad as some now claim. There is no evidence that forecasts have deteriorated over time; indeed, the opposite view—that some significant improvement has occurred—finds more support in the data, as will be shown directly. So the question "Has macro-forecasting failed?" cannot be answered in the affirmative simply on this ground.

Table 1 is based on the 1953–89 series of predicted annual rates of change in aggregate demand, output, and the price level. It covers a large number of forecasts from a variety of sources: business economists and others employed by private companies in manufacturing, finance, trade, consulting, etc.; academic and research institutions; and the president's Council of Economic Advisers (CEA), which prepares the principal government forecast. Predominantly judgmental predictions that originate mainly in business are represented along with predictions made by econometricians working with large models.[6] Although the included forecast sets differ in many respects, they are treated as sufficiently comparable for our present purpose, which is to consider the broad trends over time in the overall forecasting accuracy. The question of who forecasts best is of no interest here. All the predictions covered are made around the end of the

[6]Michigan and Wharton are the oldest families of such models in use. For a list of the coded sources, see Table 1, note a.

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TABLE 1

ANNUAL FORECASTS OF PERCENTAGE CHANGES IN NOMINAL AND REAL GNP AND THE IMPLICIT PRICE DEFLATOR: SUMMARY MEASURES OF ERROR, 1953–89

Line	Period Covered (1)	(No. of Years)	Forecasts (Code)[a] (2)	Mean Abs. Errors (MAE)[b] Mean (3)	Mean Abs. Errors (MAE)[b] Range (4)	Mean Errors (ME)[c] Mean (5)	Mean Errors (ME)[c] Range (6)	Relative Error[d] (7)	Mean Abs. % Change[e] (8)
				Gross National Product (GNP)[f]					
1	1953–76	(24)	1, 2	1.4	.4	−.8	.3	.6	6.6
2	1956–63	(8)	1, 2, 4	1.6	.3	−.4	.7	.8	5.0
3	1963–76	(14)	1, 2, 6, 7, 8	1.0	.5	−.5	.6	.6	7.9
4	1969–76	(8)	1, 2, 5, 6, 7, 8	.9	.4	−.1	.6	.5	8.4
5	1969–89	(21)	5, 6	1.1	.2	.1	.3	.6	8.4
				GNP in Constant Dollars (RGNP)[f]					
6	1959–67	(9)	3, 7	1.2	.3	−.7	.4	.7	4.3
7	1962–76	(15)	6, 7	1.2	.3	.4	.4	.5	4.1
8	1969–76	(8)	5, 6, 7, 8	1.2	.7	.7	.3	.3	3.6
9	1969–89	(21)	5, 6	1.1	.1	−.2	.2	.4	3.2
				GNP Implicit Price Deflator (IPD)[f]					
10	1959–67	(9)	3, 7	.6	.1	.1	.2	2.0	1.9
11	1962–76	(15)	6, 7	1.0	0	−.5	0	.8	4.2

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12	1969–76	(8)	5, 6, 7, 8	1.4	.1	−.8	.3	.7	5.9
13	1969–89	(21)	5, 6	1.1	.2	−.1	.2	.8	5.6

[a]1: Livingston survey, mean; 2: Mean of eight private forecasts (Harris Bank, National Securities and Research Corp., Conference Board Economic Forum, University of Missouri School of Business, UCLA Business Forecasting Project, *Fortune* magazine, IBM Economic Research Dept., Prudential Insurance Co.); 3: Mean of five forecasts (the first five listed under forecast set 2 above); 4: New York Forecasters Club, mean; 5: ASA/NBER Economic Outlook Survey, median; 6: Council of Economic Advisers (CEA); 7: Research Seminar in Quantitative Economics of the University of Michigan (RSQE Michigan); 8: Wharton School Economic Forecasting Unit, University of Pennsylvania (Wharton).

[b]MAE = $1/n\ \Sigma|E_t|$, where $E_t = P_t - A_t$; P_t = predicted value, A_t = actual value (first estimate).

[c]ME = $1/n\ \Sigma\ E_t$.

[d]Ratio of the mean MAE of forecast (column 3) to the MAE of the corresponding naive model N4 (lines 1–9) or N2 (lines 10–13). N4 projects the moving average of the last four observed changes ($1/n\ \Sigma\ A_{t-i}$, $i = 1, \ldots, 4$). N2 projects the last observed change (A_{t-1}).

[e]Computed from preliminary data (first estimates for year t published in year $t + 1$).

[f]All measures refer to annual percentage changes and are in percentage points.

SOURCES: Zarnowitz (1979, Tables 1–3 for 1953–76; author's files and calculations for 1969–89). See also Moore (1983, Tables 26.3 and 26.4), *Economic Report of the President,* and *Budget of the U.S. Government.*

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year for the year ahead. The data come from my earlier and recent work, and their availability dictates the division by subperiods shown in the table. The relative dispersion of the measures across the sets of forecasts and across time tends to be moderate, even when represented by ranges between extreme values.

Thus, the MAEs, in percentage points, average 1.2, 1.2, and 1.0 for GNP, RGNP, and IPD, respectively (column 3). The corresponding figures for the ranges are .4, .4, and .1 (column 4). Reading down the table, a comparison of the successive (mostly overlapping) subperiods suggests that the MAEs may have decreased somewhat for GNP, increased for IPD, and remained remarkably stable for RGNP. But the errors of inflation forecasts increased on average over time much less than the actual inflation rates did, so the accuracy of these forecasts improved greatly in relative terms (compare columns 3 and 8). Such comparisons also suggest a definite reduction in the relative errors for GNP, but a small increase in those for RGNP (note that the average real growth rates decreased slowly between 1959–67 and 1969–89).

It is also instructive to look at ratios of the MAEs of the forecasts to the MAEs of the corresponding extrapolations from selected naive models (column 7).[7] All but one of these relative error measures fall in the range of .3 to .8, indicating the superiority of the forecasts. The single exception is the ratio of 2.0 for the IPD forecasts in 1959–67, a period when projections of last year's rate of change in the price level were surprisingly effective because inflation was unusually low and stable.

In the 1950s and 1960s forecasters generally underpredicted the nominal and real GNP growth rates in years of cyclical expansion (i.e., most of the time). Defined as differences (predicted minus actual values), the errors in these forecasts, therefore, were on the average negative (see the mean errors [MEs] in column 5). The early postwar period enjoyed more real growth than had been expected on the basis of historical experience. Gradually and somewhat belatedly, forecasters learned to be more optimistic. Real GNP increases were strongly underestimated in 1959–67 but overestimated in 1962–76, particularly in 1969–76.

Meanwhile, the IPD forecasts had little if any bias in the period of relative price stability (1959–67); but when inflation was rising and high, it was clearly underpredicted, as in 1962–76 and especially

[7]For GNP and RGNP, projections of four-year trailing moving averages proved relatively effective; for IPD, projections of last year's observed values. Percentage changes based on preliminary data are used.

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in 1969–76. Finally, inflation peaked in 1980–81 and decreased markedly in the following five years. Predicted rates moved down with a lag, thus tending to overestimate actual rates. Later inflation increased again but slowly, which was well anticipated.

Real GNP growth was underestimated in 1980, when the recession turned out milder than expected, and in the years of strong recovery and expansion (1983–84, 1988); it was greatly overestimated in 1982, after a severe downturn cut short an unusually weak and brief rise in activity. Thus, as was often observed in the past, the largest errors were associated with business cycle turning points and recessions as well as sharp accelerations and decelerations in inflation (more about this later). However, more than half the time, the annual forecasts for all three variables had only moderate errors of less than one percentage point; also, on the whole the underestimates and overestimates balanced each other well, as can be seen from the results for 1969–89 (lines 5, 9, and 13).

Over some shorter subperiods, the overall mean errors are much larger and so are some of the associated dispersion measures (columns 5–6). Thus it may seem that some of the included forecast sets show undesirable bias (i.e., persistent underprediction or overprediction suggestive of a failure to learn from past errors). The requirements that forecasts should be unbiased and also efficient (uncorrelated with their own errors) are often treated as obvious and minimal in the literature. Yet such requirements are based on assumptions that are only too often falsified in practice, namely that the patterns and relations of the variables concerned are approximately time-invariant and known.

In reality, economic processes are not necessarily stable as they reflect changes in economic institutions, structure, policies, and behavioral rules. Forecasting models and techniques are adapted and altered frequently, and many of the available samples of consistent predictions are too small to establish the existence and to evaluate the importance of any systematic errors. Also, measurement errors may distort and fragment both the time-series data and the related forecasts. For any or all of these reasons, ex post tests can and do find evidences of bias, even in some state-of-the-art predictions from respected sources that ex ante had much professional approval. It seems unlikely that these forecasts were, in fact, systematically deficient in the sense of having persistent yet avoidable errors. I suspect their errors are mainly period-specific and are of the kind that could not be readily detected and corrected at the time the forecasts were made.

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Finally, we should note that the forecasts represented in Table 1, though not selected retrospectively for being particularly accurate, have not been randomly chosen either and are certainly superior to many other forecasts. One reason is that I wished to include the important sources with long records such as CEA, Michigan, and Wharton. Another is that the group averages from surveys of forecasters, such as ASA/NBER, are always more accurate over time than most of their individual components (Zarnowitz 1984, 1985). Such averages also conceal the dispersion of errors, which is often large, in the participants' forecasts. The measures presented here and in some other studies suggest that the forecasts by government agencies and teams of econometricians equipped with large-scale models are about as accurate as the survey group averages that represent predominantly judgmental predictions by private business economists.[8]

Tougher Forecasting Tasks and Criteria

In the second post–World War II decade and thereafter, macroeconomic forecasts in the United States grew not only much more abundant but also much bolder. The range of the predicted variables increased greatly as more complete and detailed models were built; it came to include important but very volatile—and hence difficult-to-predict—time series such as corporate profits, housing starts, and inventory investment. Moreover, forecasters were increasingly called upon and able to satisfy the demand for multiperiod predictions of the economy's course. Such forecasts regularly extend over sequences of one to two years and four to eight quarters ahead (some are even longer). This "dynamic" type of forecasting is particularly ambitious. Of course, the computer revolution had much to do with these developments, but so did the advances in economics and statistics, along with the trends in government and business managements.

The average accuracy of forecasts typically decreases as their horizon increases (e.g., GNP is predicted better one quarter than two quarters ahead, better two than three quarters ahead). That is, the MAEs (and RMSEs) rise with each extension of the predictive span, from the current quarter t (for series where preliminary data appear only in the next quarter $t + 1$) through several quarters of the near future. However, the margins by which the absolute or squared errors

[8]Econometric service bureaus usually adjust many predictions generated by their models in attempts to use judgment and up-to-date outside information to correct for errors that an unaided model would commit. The net effects of these constant-term adjustments have been mostly to improve the accuracy of forecasts by compensating partially for the errors in the models and the projected values of the exogenous variables (Evans, Haitovsky, and Treyz 1972; Zarnowitz 1972; McNees 1990).

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accumulate over time tend to decrease, and beyond a certain point (often $t + 4$ or $t + 5$) the errors often flatten or vary irregularly around some high plateau.[9] Current information and knowledge may help us forecast $(t + 2)$ better than $(t + 4)$, but we may be about equally ignorant about $(t + 8)$ and $(t + 10)$, for example. These observed tendencies apply to short-term forecasts for a variety of time series; they apply to levels and cumulative absolute and relative changes alike.

Properly understood, the rule "longer forecasts, larger errors" applies to optimal forecasts. It should be a strong regularity—and is. Each of several potential ingredients of a forecast—extrapolation of time series and their relations, external information, or judgment—is subject to a deterioration with the lengthening of the predictive span. Annual forecasts can be viewed as having average spans of about two and a half quarters from the date of issue to the midpoint of their target period; they tend to be about as accurate as comparable forecasts for two or three quarters ahead, less accurate than shorter and more accurate than longer forecasts. Errors in predicting the intrayear quarterly changes often offset each other, which helps considerably the annual forecasts of nominal and real GNP and some of their components. (On the other hand, the multiperiod forecasts of inflation have more cumulation and fewer offsets).

It is not possible or necessary to go beyond this summary of the main features of the relation between the accuracy and the horizon of forecasts. Substantial research has been done on these matters, and its findings are generally consistent with and supportive of the above story (see, e.g., Zarnowitz 1979 and McNees 1988). Clearly, it is much more difficult to predict sequences of quarterly values than single annual values, and the usefulness of point forecasts with long spans and large errors is in doubt. But it is also clear that the tough task of quarter-by-quarter forecasting of the near-term course of the economy in some detail has now become something of a professional routine, presumably in response to the rising demand for just such forecasts. The practice of monthly updating of the forecasts spread concurrently and fast, too: It gives users much fresh information about the outlook for the economy and gives forecasters the opportunity to revise their predictions frequently.

[9]In other words, the errors increase less than in proportion to the horizon (e.g., semiannual predictions are less than twice as accurate as the annual ones). Indeed, the errors frequently *decrease* with the lengthening horizon for forecasts of growth in the nominal and real aggregates when these are expressed throughout at *annual rates*.

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One question that arises at this point is whether the multiperiod predictions are superior to matching mechanical extrapolations from naive and time-series models on the basis of past data available at the time of forecast. Table 2 compares the RMSEs of the forecasts (P) with their counterparts for several naive models (N): projections of last-known levels, changes, and historical averages (N1, N2, and N2*, respectively) and autoregressive extrapolations (N3). The listed ratios RMSE(P)/RMSE(N) are all less than 1, which indicates that the average accuracy of the forecasts is generally higher than that of the naive models. However, in some instances the extrapolations are not much worse than the forecasts: Notably, of the six pre-1970 sets, two have ratios of .9 and higher for the two-quarter span and four for the four-quarter span (lines 1–6, columns 6 and 7). But the best forecasts scored well against the naive models in the early 1970s, a period of great turbulence (lines 7–9). Overall, this fragmentary record illustrates a fairly good but not particularly impressive forecasting performance.

Recently, macro-forecasters have been challenged to exceed higher standards represented by predictions based on vector autoregressive (VAR) models (Sims 1980). In a VAR, each of the variables is predicted by regression on its own lagged values and those of the others; none are exogenous. The number of variables is small, since each is used with several lags. The only use of economic theory and judgment is in choosing the variables. The forecasting process itself is mechanical and replicable. In contrast, econometric forecasting involves exogenous variables that are projected outside the model and, typically, judgmental adjustments to the model outputs of endogenous variables. At least potentially, the roles of both theory and judgment in macroeconometric modeling and forecasting are very large.

Table 3 shows ratios of the average RMSEs of Chase, DRI, and Wharton econometric forecasts to the corresponding measures for predictions from a VAR model by Lupoletti and Webb (1986). The model consists of five variables—RGNP, IPD, the monetary base, the manufacturing capacity utilization rate, and the 90-day treasury bill rate (TBR)—each taken with six lags. It was estimated for 1952:2 to 1969:4, and the obtained coefficients and predictions were then used to forecast each variable for 1970:1 to 1972:2. This procedure was repeated starting with each successive quarter to produce forecasts with horizons of one to six quarters for 1970:1 to 1983:4. Thus, the results are postsample predictions intended to be comparable in this respect to the authentic ex ante forecasts.[10]

[10]However, the data used in the VAR computation were the latest revised estimates

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For GNP and RGNP, the RMSE ratios have a range of .73 to .97; for IPD and TBR, half of the ratios favor the forecasts and half favor VAR, but all are close to 1 (see lines 1, 4, 7, and 10 in Table 3). Thus, in most cases the forecasts appear to be more accurate than the much simpler and less expensive VAR projections, but by small margins. The mixed results of comparisons by subperiods (not shown) confirm this conclusion. Perhaps surprisingly, in most cases the relative performance of VAR improved at longer horizons, although prominent econometricians would expect the opposite (Klein 1984, p. 7; Adams 1986, p. 156).

Other evaluations showed that the published forecasts with macroeconometric models more often outperformed univariate ARIMA and multivariate VAR times-series models (McNees 1982; Wallis et al. 1987 for the United Kingdom). In any event, such extrapolations make good competitive standards against which to assess the accuracy of sets of predictions produced by serious and aspiring forecasters.

Overfitting is a major problem for a VAR model that typically includes many terms (their number equals the product, variables times lags) and hence requires estimation of many parameters from a limited amount of data. To avoid or at least reduce this difficulty, Bayesian vector autoregressions (BVAR) use selected restrictions (e.g., that the prior means are one for the coefficients on the first own lag, zero elsewhere, and that the standard deviation of the independent normal distribution for the j-th lag is inversely proportional to j (Litterman 1984, 1986). Thus, the priors contain elements of random-walk models, but the approach is flexible in that it uses alternative proportionality (tightness) specifications and time-varying parameters. McNees (1986) presents detailed comparisons in terms of RMSEs between the regular ex ante BVAR forecasts issued by Litterman in 1980–85 and some of the best-known forecasts by econometricians armed with large-scale models and averages from surveys by business economists. He reports that BVAR forecasts were the most accurate or among the most accurate for RGNP, for the unemployment rate, and for real nonresidential investment and were the least accurate for IPD and (by very small margins) TBR; for GNP, their record was relatively weak over the short spans, strong over the long spans. Table 3 presents a summary of some of this evidence (see lines 3, 6, 9, and 12).

available to the authors, whereas the econometric services used the preliminary estimates available at the time of the forecast. This use could well bias the comparisons considerably in favor of VAR, although Lupoletti and Webb (1986, Table 1 and text, pp. 367–69) present some evidence that this may not be the case.

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TABLE 2

SEMIANNUAL AND QUARTERLY MULTIPERIOD FORECASTS OF SIX VARIABLES: COMPARISONS WITH SELECTED NAIVE MODELS OVER SPANS OF ONE TO EIGHT QUARTERS, 1947–75

Line	Variable (Level or Change)[a] (1)		Period Covered[b] (2)	Forecast Sets Covered[c] (3)	Benchmark Model[d] (4)	Span of Forecast, in Quarters				
						One (5)	Two (6)	Four (7)	Six (8)	Eight (9)
						Ratio: RMSE of Forecast to RMSE of Naive Model[e]				
1	IP	(L)	1947–63	NYFC	N1	—	.71	.80	—	—
2	IP	(L)	1956–63	Fortune	N2*	.68	.62	.92	.80	—
3	GNP	(Δ)	1953–63	Fortune	N3	.70	.74	.96	—	—
4	GNP	(%Δ)	1958–69	IBM	N2	.66	.91	.70	—	—
5	PCE	(%Δ)	1958–69	IBM	N2	.82	.70	.93	—	—
6	GPDI	(%Δ)	1958–69	IBM	N2	.78	.90	.97	—	—
7	GNP	(%Δ)	1970–75	Six Sets	N1	.26	.28	.26	.21	.17
8	RGNP	(%Δ)	1970–75	Six Sets	N1	.45	.55	.67	.69	.73
9	IPD	(%Δ)	1970–75	Six Sets	N1	.32	.34	.40	.43	.48

[a]IP = index of industrial production; PCE = personal consumption expenditures; GPDI = gross private domestic investment; GNP, RGNP, IPD—as used in Table 1; L = level; Δ = change; %Δ = percentage change.

[b]Number of observations per span: line 1, 33; line 2, 13; line 3, 20; lines 4–6, 37, 34, 21 (in columns 5–9, respectively); lines 7–9, 22, 21, 19, 17, and 12 (in columns 5–9, respectively).

[c]NYFC = New York Forecasters Club; Fortune = "Business Roundup" in *Fortune* magazine; IBM = IBM Corp., Economic Research Dept. Six sets = ASA/NBER, BEA, Chase, DRI, GE, and Wharton, mean ratio (BEA = Bureau of Economic Analysis in the U.S. Department of Commerce; Chase = Chase Econometric Associates, Inc.; DRI = Data Resources, Inc.; GE = General Electric Co.; Wharton = Wharton Econometric Forecasting Associates, Inc.). All forecasts are quarterly, except NYFC, which is semiannual.

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[d]N1 projects the last known value of the given variable, A_{t-1}; N2, the last known change, ΔA_{t-1}; N2*, the last known average historical change, ΔA. N3 refers to a five-term autoregressive extrapolation (where A_t is regressed on A_{t-i}, $i = 1, \ldots, 5$). Entries in lines 7–9 were computed as Theil's inequality coefficients U ($U = 1$ for the naive model projecting the last known value of the series, here in %Δ).

[e]RMSE = root mean square error. The naive model applied in each line is identified in column 4 (see note d). The entries in columns 5–7 are means of the RMSE ratios for six forecasts each; those in columns 8 and 9 are means of the RMSE ratios for four forecasts each (ASA/NBER and BEA forecasts do not range beyond four future quarters).

SOURCES: Lines 1 and 2, Zarnowitz (1967, Table 18, pp. 100–101); line 3, Mincer and Zarnowitz (1969, Table 1–8, p. 42); lines 4–6, Zarnowitz (1974, Table 6, p. 581); lines 7–9, Zarnowitz (1979, Tables 5, 6, and 7, pp. 18, 20, and 22).

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TABLE 3

QUARTERLY MULTIPERIOD FORECASTS OF FOUR AGGREGATE VARIABLES: COMPARISONS WITH VAR AND BVAR MODEL FORECASTS, 1970–85

Line	Period Covered[a] (1)	Forecast[b] (2)	Benchmark Model[c] (3)	Span of Forecast in Quarters: One (4)	Two (5)	Four (6)	Six (7)	Eight (8)
				Ratio: RMSE of Forecast to RMSE of Benchmark Model				
Gross National Product (GNP)								
1	1970–83	Three sets	VAR	.82	.81	.97	.94	—
2	1970–83	Same, TP	VAR	.88	.98	1.48	1.41	—
3	1980–85	Eleven sets	BVAR	.86	.88	.88	.95	1.06
GNP in Constant Dollars (RGNP)								
4	1970–83	Three sets	VAR	.77	.73	.86	.92	—
5	1970–83	Same, TP	VAR	.81	.86	1.04	.96	—
6	1980–85	Eleven sets	BVAR	.93	1.12	1.29	1.74	1.34
GNP Implicit Price Deflator (IPD)								
7	1970–83	Three sets	VAR	.83	.95	1.00	1.00	—
8	1970–83	Same, TP	VAR	.73	.78	.88	.92	—
9	1980–85	Eleven sets	BVAR	.78	.56	.47	.47	.57

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90–Day Treasury Bill Rate (TBR)

10	1970–83	Three sets	VAR	1.11	1.02	.98	.96	—
11	1970–83	Same, TP	VAR	1.19	1.12	1.09	1.10	—
12	1980–85	Eleven sets	BVAR	.90	.91	.97	.83	.77

[a]Numbers of quarterly observations for 1970–83: 53, 52, 50, and 48 in columns 4, 5, 6, and 7, respectively; for 1980–85: 20, 19, 17, 15, and 13.
[b]The three sets include Chase Econometrics, Data Resources, and Wharton Econometric Forecasting Associates (lines 1, 2, 4, 5, 7, 8, 10, and 11). TP refers to forecasts made within two quarters of a business cycle turning point as identified by the National Bureau of Economic Research (NBER). The 11 sets include ASA/NBER median; BEA; Chase; DRI; RSQE, University of Michigan; UCLA (University of California at Los Angeles, School of Business); Wharton; Economic Forecasting Project, Georgia State University; Kent Econometric and Development Institute; Manufacturers Hanover Trust; and Townsend-Greenspan & Co., Inc.
[c]VAR = a five-variable, six-lag quarterly vector autoregressive model. The five variables are the monetary base, the real GNP, the implicit price deflator (all expressed as percentage changes), the manufacturing capacity rate (level), and the 90–day treasury bill rate (level). VAR forecast for 1990:1 is based on data for 1952:2–1969:4, etc. BVAR = Bayesian VAR (see Litterman 1984, 1986).

SOURCES: Comparisons with VAR are in Lupoletti and Webb (1986, Table 3–6, pp. 272–73); those with BVAR are in McNees (1986, Tables 1, 2, 5, and 6, pp. 25–29).

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Econometric and judgmental forecasts are commonly believed to have an advantage over time-series forecasts in that they are at least potentially much better equipped to predict turning points. This belief is generally correct for simple forms of extrapolation, but apparently not always for sophisticated time-series models. Table 3 suggests that the BVAR model was often more accurate than the econometricians within two quarters of business cycle turns in 1970–83 (lines 2, 5, 8, and 11).

Overall, the main lesson here is again that the BVAR models include information of predictive value that is not present in the econometric and judgmental forecasts; hence none of these types of predictions are systematically more or less accurate than the others. However, combining the information they contain can improve the forecast. On this point there is general agreement among studies that otherwise offer somewhat different assessments of the relative accuracy of time-series versus econometric forecasts of macroeconomic variables.[11]

Forecasting and the Business Cycle

Forecasters tend to undervalue cyclical movements. The levels of GNP and industrial production, for example, are underestimated most early in a business recovery when growth is particularly strong, less so later when the expansion slows. In general retardations and contractions, the predicted levels as a rule exceed the actual ones, either because the downturn is missed or because the decline turns out to be larger than was forecast.

There are two types of directional errors: a "missed turn," when a turning point in a series occurred but was not predicted, and a "false signal," when a turning point was predicted but did not occur. Since GNP grows most of the time and is expected to, false signals are rare in annual forecasts for this series, but missed turns (as a rule, peaks) are more frequent (Table 4, lines 1–4, column 1). For industrial production (a much more cyclical and volatile series), the percentage of missed turns was smaller and that of false signals much larger; for the GNP implicit price deflator, both relative frequencies were higher yet (lines 1–4, columns 2 and 3).

Quarterly series include many more turning points (both cyclical and minor) than the corresponding annual series. The early multiperiod forecasts of GNP missed most of the declines in the current

[11]Compare McNees (1982, 1986); Lupoletti and Webb (1986); Wallis (1989); Holden and Broomhead (1990, for the United Kingdom); and particularly Fair and Shiller (1990).

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TABLE 4

FREQUENCIES OF TURNING POINT ERRORS AND THE ACCURACY OF FORECASTS IN BUSINESS CYCLE EXPANSIONS AND CONTRACTIONS: SELECTED MEASURES FOR FOUR SUBPERIODS, 1947–89

Line	Statistic[a]	Annual Forecasts, 1947–65[b]			Quarterly Forecasts of GNP, 1955–63[c]				
		GNP (1)	IP (2)	IPD (3)	0–1 (4)	1–2 (5)	2–3 (6)	3–4 (7)	5–6 (8)
1	No. of forecast sets	12	11	5	3	3	3	3	3
2	No. of observations	126	127	78	47	45	44	38	39
3	% of actual TP missed	26	15	39	75	83	100	100	100
4	% of predicted TP false	6	18	22	17	13	6	5	—

		Annual Forecasts, 1969–89[d]			Quarterly Forecasts, 1971:2–1985:1[e]			
		GNP	RGNP	IPD	GNP	RGNP	IPD	UR
5	MAE, b.c. expansions	0.9	0.8	0.9	1.8	1.2	1.1	0.6
6	MAE, b.c. contractions	1.5	1.9	1.8	3.0	3.0	2.5	1.0

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TABLE 4 (cont.)

FREQUENCIES OF TURNING POINT ERRORS AND THE ACCURACY OF FORECASTS IN BUSINESS CYCLE EXPANSIONS AND CONTRACTIONS: SELECTED MEASURES FOR FOUR SUBPERIODS, 1947–89

		Annual Forecasts, 1969–89[d]			Quarterly Forecasts, 1971:2–1985:1[e]			
Line	Statistic[a]	GNP	RGNP	IPD	GNP	RGNP	IPD	UR
7	ME, b.c. expansions	−0.2	−0.2	0.0	−0.8	−0.4	−0.3	0.1
8	ME, b.c. contractions	1.0	1.4	0.4	−2.4	2.9	−0.8	−1.0

[a]TP = turning points; MAE = mean absolute error; ME = mean error; B.C. = business cycle.

[b]The 12 sets for GNP include 10 forecasts covered in Table 1, line 2 (as listed there in note a) plus averages from two additional large groups, the F. W. Dodge survey of economists and forecasts tabulated annually by the Federal Reserve Bank of Richmond. The 11 sets for industrial production (IP) include the same sources as for the GNP forecasts except Prudential Insurance Co. The 5 sets for IPD include the NICB (Conference Board) Forum, Harris Bank, University of Missouri Business School, UCLA Business Forecasting Project, and National Securities and Research Corp.

[c]Three sets of forecasts are covered: IBM Corp., Economic Research Dept.; New York Forecasters Club; and *Fortune* magazine. 0–1 refers to the change from the current to the next quarter (t to $t + 1$); 1–2 refers to the change from the first to the second future quarter ($t + 1$ to $t + 2$); etc.

[d]Two sets of forecasts are covered: Council of Economic Advisers (CEA) and the ASA/NBER *Economic Outlook Survey*, median (fourth-quarter forecasts for the next year). The years of business contraction, including troughs, are 1970, 1974, 1975, 1980, and 1982 (the other 16 years between 1969 and 1989 are years of business expansion, including peaks).

[e]Five sets of forecasts are covered: ASA/NBER survey median, Chase, DRI, Wharton, and BEA. Expansions, including peaks, cover quarters 1971:2–1973:4, 1975:2–1980:1, 1980:4–1981:3, and 1983:1–1985:1. Contractions, including troughs, cover quarters 1974:1–1975:1, 1980:2–1980:3, and 1981:4–1982:4. UR = unemployment rate.

SOURCES: Lines 1–4, Zarnowitz (1974, Table 7, pp. 588–89; lines 5–8, columns 1–3, author's files and calculations; lines 5–8, columns 4–7, Zarnowitz (1986, Table 2, p. 24).

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and next quarters and all of the declines in the more distant quarters ahead (i.e., they falsely predicted rises instead). In contrast, false signals (defined here as predictions of decreases when increases actually occurred) were relatively few and fading with the distance to the target quarter (lines 1–4, columns 4–8).

Real GNP turned down in 1954, 1958, 1970, and 1974. Of the 10 forecasts for these years that were available to me for study, 8 specified continued rises and only 2 succeeded in signaling declines (Zarnowitz 1979, p. 10). Even though they are usually few and far between, cyclical turning-point errors matter greatly because they tend to be exceedingly large. Thus, on average, they are about three times larger than the other errors in forecasts of annual percentage changes in real GNP, as shown in Table 5.

During the 1970s and 1980s, the largest errors in real GNP forecasts occurred in years of business cycle recession and troughs, namely 1970, 1974, and 1982; all were positive. Thus, the forecasts continued to suffer from the failure to predict downturns in aggregate economic activity, even though their relative accuracy improved and the frequency of directional errors decreased compared with the earlier post–World War II period.

A widely observed and strong property of forecasts is that they are more accurate and less biased during periods of business expansion (including peaks) than during periods of contraction (including troughs). When the economy keeps rising, its course is predicted with substantially greater accuracy than when it falls. This result is shown by both the mean absolute errors and the mean errors (disregarding sign), and for both the annual and the quarterly forecasts (Table 4, lines 5–8).

A similar, though somewhat muted, contrast exists between forecasts for the above-average growth phases and those for the below-average growth phases: The former are on average more accurate and less biased than the latter.[12] In sum, large errors tend to cluster around and immediately after business cycle turns and growth cycle turns, especially peaks (growth cycles are major fluctuations in trend-adjusted aggregates).

Note that the meaning of these results is not simply that the forecasting failures are due to large unanticipated shocks, for the latter can and often do occur under any economic conditions. The concentration of large errors during slowdowns and contractions cannot be explained away by a general reference to random outside disturbances. The economy is particularly vulnerable in these business

[12]For evidence, see Zarnowitz (1986, Table 2).

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TABLE 5

FORECAST ERRORS OF ANNUAL PERCENTAGE CHANGES IN REAL GNP

	Underestimates		Overestimates		Turning Point Errors	
	No.	MAE	No.	MAE	No.	MAE
Five sets, 1959–76[a]	33	1.1	21	.9	8	2.8
CEA, 1969–89	8	.9	9	.8	3	2.3
ASA/NBER, 1969–89	9	1.2	8	.5	3	2.8

[a]Includes the first 5 sets listed under code 2 in Table 1, note a. For further details, see Zarnowitz (1979, Table 2, col. 1).

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cycle phases owing to a gradual accumulation of various stresses and imbalances, and it is very difficult to predict just when these phenomena will culminate. Also, few forecasters take the risk of signaling a recession prematurely ahead of others; the costs of such prediction to themselves and their customers can be quite high. In contrast, forecasts of a recovery are always welcome and often accepted on the basis of early signs of improvement. The peak errors show up during the recession and slowdown periods, the generally smaller trough errors show up during the recovery and speedup periods.

The most influential private forecasts are now issued monthly for sequences of several quarters ahead; government forecasts no doubt are adjusted just as frequently, though only for internal uses, not public knowledge. How early can the alert producers of such predictions foresee or detect major events such as the turning points in business cycles, growth cycles, and inflation fluctuations? Experience varies but the probable lead times at peaks, if any, are short. For example, the first forecasts of a downturn in 1973 were coincidental with the onset of the recession in the fall; the many predictions of a peak in 1979 found much support in preliminary data but were not confirmed until 1980:1; the mid-1981 peak was widely missed; and few predicted a decline in 1990 before August, though soon thereafter most forecasters agreed that a recession was under way.[13]

Most of the recent business contractions were preceded by fairly long slowdowns in aggregate economic activity (1957, 1969, 1973, 1979, 1989–90). A number of leading indicators and corresponding composite indexes declined or flattened early on each of these occasions, providing early signals that the economy was weakening. Many forecasters, monitoring these developments, promptly recognized the slowdowns but discounted the associated recession risks. As a result, the forecasts, like the indicators, tend to have a better record of timely prediction for slowdowns (growth cycle peaks) than recessions (business cycle peaks).

At business cycle troughs, the leads of the indicators are generally much shorter than at peaks but are also much less variable. Thus, forecasters may trust these signals more, but they are too often ready to predict the end of a recognized recession much earlier. So, the prevailing view in spring 1974 (after the oil embargo ended) was that the recession was about over, but that view was premature by almost a year. Similarly, most forecasters expected the 1981–82

[13]For detail, see McNees (1990, pp. 159–67) and (on the 1990 peak forecasts) Zarnowitz (1990, 1991).

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contraction to last two to three quarters rather than the actual five quarters. (But this error was in part related to the opposite error of overestimating the length of the 1980 recession, which was unusual in lasting only six months.)

The turning points marking the major rises and declines in inflation have been for the most part poorly predicted, with forecasts lagging behind the actual values much as simple extrapolations would. A detailed analysis of a large number of forecasts for several variables found inflation errors particularly troublesome; so have some other studies (Zarnowitz 1985; Holden and Peel 1985 for the United Kingdom).

Conclusion

There is much disenchantment with macroeconomic forecasting. The difficult question is "How much is due to unacceptably poor performance and how much to unrealistically high expectations?" I would argue that the latter is a major factor. Economists were held in high repute during the 1960s, probably in large part because the macro-forecasts looked good then, and high growth and prosperity prevailed for some time with inflation still well restrained. But it is relatively easy to achieve a respectable forecasting record in times of continuing expansion. Later, when inflation accelerated, when serious recessions reappeared, and when long-term growth of productivity and total output slackened, the errors of macro-models and macro-forecasts received increased public attention—as did the old and new controversies among the economists. The reputation of the profession suffered and, perhaps worse yet, the interest of academic economists in forecasting, never very strong, weakened still more. Yet the performance of professional economic forecasters, when assessed in proper relative terms, has been considerably better in recent times than in the earlier post–World War II period. What happened is that the improvements fell short of enabling the forecasters to cope with the new problems they faced.

As a practical activity, the results of which are marketed, recorded, researched, and tested, macroeconomic forecasting is very young by any standard. There is little doubt that it will always disappoint the hopes of many, but also a high probability that it can be developed well beyond its present early stage. If macroeconomics has a long way to go, as I believe to be the case, then macro-forecasts, too, should still be far from the limits on their improvability, even if such limits were to prove much narrower than early enthusiasts thought.

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Progress in forecasting will require chiefly better data and models, but also improvements in time-series analysis, econometric methods, cyclical indicators, and anticipations surveys. These essentially complementary tools should be used efficiently as such, not as competitors or substitutes. We can expect the advances to prove difficult and slow with setbacks along the way. Indeed, some large banks and industrial companies have sharply reduced or even liquidated their economic staffs in recent years. But this move can hardly mean that these organizations have suddenly discovered that they can do without forecasts of important aspects of aggregate economic activity on which their own business prospects may depend critically. More likely, they decided that other ways to acquire such forecasts (e.g., subscription to outside services or surveys) are more economical than in-house production. The predictive needs of decisionmakers who are necessarily future-oriented are not reduced by the perceived shortcomings of past forecasts. This view applies to government policymakers as well as to private agents.

However, there are ways to reduce one's dependence on forecasts to a degree, and the incentives to use them are presumably greater the more fallible the forecasts appear. One partial substitute for the lacking foresight is readiness to respond promptly and flexibly to unforeseen changes. For example, private reactions to economic fluctuations may involve employment smoothing through labor hoarding along with production smoothing through changes in unfilled order backlogs.[14] An example of a government policy that is relevant here would be job creation through countercyclical public works or public service employment programs, which would draw on an advance preparation of a backlog of useful projects to be activated as needed (compare Council of Economic Advisers 1954, p. 123). But fiscal policies of this kind were more often pro-cyclical than counter-cyclical because of tardiness and poor planning (see Zarnowitz and Moore 1982, pp. 57–59).

The principal proposed alternative to discretionary government actions that must rely on forecasts is to consistently follow a stable policy rule that would call either for no response or for a predetermined response to changes in the economy (e.g., a fixed growth rate for some controlled monetary aggregate or a rate varied as a function of, say, the observed inflation). Policy rules, it is often argued, can be expected to have positive stabilizing effects on private expectations

[14]Also, theoretically, through changes in inventories of produced goods; but empirically, inventories do not seem to be used to accommodate cyclical fluctuations in demand (see, e.g., Blinder 1986).

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and to discipline the authorities that may otherwise be tempted to engage in shortsighted attempts to overstimulate the economy by inflationary policies. Thus, the deficiency of forecasts is not the only argument used in favor of the rules; nor is it necessarily the main one. Monetary control could be poor even with accurate forecasts because of inconsistent and inefficient procedures with regard to the instruments and targets of the policy.

Are even the best available forecasts inadequate to be a basis for satisfactory policy decisionmaking? This question probably does not have a single, clear-cut answer at this time. There is evidence that the government has no substantial and lasting informational advantage; notably, the CEA and the Federal Reserve forecasts are about as accurate as the state-of-the art private forecasts (as indicated by our results and other studies; see, e.g., Meltzer 1991, pp. 30–32). Certainly, the forecasts cannot support "fine-tuning," that is, keeping the economy always very close to full employment; but this would not be a realistic goal for balanced policies even if macroeconomic forecasting were in far better shape than it presently is. Yet forecasts should be sufficient most of the time to assist in the pursuit of reasonable policy objectives: preventing or at least effectively combating persistent high unemployment and persistent high inflation.

The main defects of macro-forecasts from the point of view of policy are the errors of missing cyclical turns and shifts in the average rates of inflation. Major reductions in such errors should rank high on the agenda of economists.

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Criteria for Judging Macroeconomic Forecasting

Allen Sinai

Victor Zarnowitz concentrates on a limited dimension of the question that titles his paper, "Has Macro-Forecasting Failed?" That dimension is macroeconomic forecast accuracy and the forecast performance of macroeconomic projections by different types of forecasters, different forecast methods, and across different periods. As a paper on the forecast accuracy of macroeconomic projections and the comparative forecasting record of various methods of macro-forecasting, the Zarnowitz paper has much to offer on four counts: (1) an evaluation of the record from 1969 to 1989; (2) how judgment-type forecasting, model-based forecasts, and time-series methods do vis-a-vis one another; (3) some myths surrounding common perceptions about macro-forecasting; and (4) some of the underlying nitty-gritty of how macro-forecasting is done.

Zarnowitz does perpetuate a few incorrect notions about macro-forecasts and macro-forecasting, however. And there is excessive material that is extraneous to the purpose of the paper and to the limited aspect of the question he, in fact, examines. But, by and large, if one takes forecast accuracy and performance as the major criteria of whether macro-forecasting has failed, the paper makes a significant contribution.

Has Macro-Forecasting Failed? How Can We Tell?

The major deficiency is that Zarnowitz never directly comes to grips with the question that is posed: Has macro-forecasting failed?

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A full and comprehensive list of criteria relating to the methods, uses, and accuracy of macro-forecasting is not laid out, on which the pluses and minuses of macro-forecasting could be examined. There is casual reference to some additional criteria, but that reference disappears as Zarnowitz pursues the accuracy issue and the empirical data in his very impressive database of historical macro-forecasts from different types of forecasters and different forecast organizations.

Even on the accuracy criterion, though, I could not tell what Zarnowitz concluded from his examination of the evidence, Unless, as he hinted, his task was simply to lay out the facts on forecast accuracy and methods as information input for the reader to render a judgment. But my real criticism is that to evaluate the failures and successes of macro-forecasting and the methods various practitioners use to advance the activity, a much broader kind of examination than the one taken by Zarnowitz is required.

The historical record of macro-forecasts and macro-forecasting is a necessary dimension on which to consider whether macro-forecasting has failed. But it is far from sufficient in light of the multitude of uses and applications—both actual and potential—of macro-forecasting. Indeed, it could be argued that in a microeconomic, bottom-line, decisionmaking context, the value of macro-forecasting and its success or failure goes far beyond the accuracy of point forecasts, or the rate of growth of GNP, or the rate of inflation and their turning points.

Point forecasts based on conditional assumptions and conditional probabalistic models must always be wrong. Focusing on accuracy, therefore, tends to distract and obfuscate the best and most profitable uses and decisions that relate to macro-forecasting in government, business, and financial institutions. For all its errors in accuracy and difficulties that are presented in use, would we ask whether meteorology has failed? Macro-forecasting, I would submit, does better and is more useful and necessary for business decisions than is meteorology.

Accuracy and Other Criteria

If I levy this criticism, how might macro-forecasting be judged as a success or failure? Without going into detail, let me list and comment briefly on some of the criteria I think are valid in answering the question, "Has macroforecasting failed?"

The Accuracy Issue

First, there is the accuracy issue, which is important—a question of levels, changes, and turning points in forecasted variables compared

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with actual values. The postwar record here is mixed. Zarnowitz does very well in presenting it plus helping users of the macro-forecasts understand the biases so that appropriate adjustments can be made. For example, it should be understood and well known that macro-forecasting always understates upswings and downswings. It is the nature of the beast, and Zarnowitz brings that point out well. The data are seasonally adjusted, models tend to add smoothing, and forecasters smooth the smooth results of the models and the data. It is really in the culture of what goes on in macro-forecasting that one gets underestimates of both the upswings and downswings. But forecasts tend to improve over time, as Zarnowitz notes, especially in relation to the magnitudes and difficulties at the time of forecasting. The oil shock of 1973–74 was a new event: it increased both inflation and unemployment. Was macro-forecasting a failure because policymakers did not fathom the implications of the oil shock or engage in the right policy response? One might come to that conclusion if the focus is on forecast accuracy and point forecasts, which distract users from maximizing for decisions the information content of the forecasts.

Wrong forecasts, however, do not necessarily mean bad decisions or bad bottom-line results. Point forecasts and forecasts of turning points will always be wrong, since neither the science of macro-forecasting nor its practitioners can really produce point forecast results on turning points or on specific variables. Once the limitations of macro-forecasting are recognized, the informational use of such forecasting is enhanced.

Other Criteria for Judging Macro-Forecasting

There are many other criteria besides accuracy for judging macro-forcasting. One criterion is the use of macro-forecasts as a backdrop for micro-decisions. It is like forecasting the weather. Macro-baseline expectations and alternative prospects are essential ingredients for virtually all micro-unit decisions and decisionmakers. That is an intelligent and successful use of macro-forecasting. Decisionmakers in government and business use macro-forecasts as benchmarks, but those benchmarks are revised as new information comes in. Again, the point forecasts cannot be taken literally. They can be used as initial guides to macroeconomic policy and to form expectations in financial markets. When a forecast fails, or is not accurate, policymakers can react.

Another criterion by which to judge macro-forecasting is how well it enhances our understanding of the macroeconomy. Today macro-results and model results of various policies are examined thoroughly

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through macro-forecasting. This can make for less chance of a really bad and poorly timed policy action. Policies are now examined in a way that never was true before. The examination of the stock market crash of October 1987 indicated no recession for most of those who ran the numbers through systematic models. For me, when I did that, that was a surprise, but a result from the discipline of the macro-forecasting process regardless of any forecast.

A further dimension is the increase in current-quarter monitoring as part of macro-forecasting. There is now almost continuous forecasting and monitoring of the near term as an input to decisions, that is, as a monitor in terms of deviations from the forecast or from expectations. That development is more important for bottom-line decisions and good policymaking than, I believe, the forecasts themselves.

Finally, in thinking about relevant criteria for determining whether macro-forecasting has failed, one must consider what can be reasonably expected from the forecasts and the forecasters. Perfect accuracy is impossible and should not be the sole criterion upon which to judge the success of macro-forecasting.

Conclusion

Zarnowitz has made a significant empirical and scientific contribution on the accuracy record of macro-forecasts in the postwar period. He also has increased our understanding of how macro-forecasts can be used. But I do not think he has begun to approach the more general title of his paper. I would say that over the postwar period, allowing for the criteria I have laid out, there are pluses and minuses—and I would be hard put to say that macro-forecasting has failed once all of its uses, actual and potential, are considered.

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The Limits of Macroeconomics

Roger W. Garrison

Introduction

There is no disillusionment in learning that any field of study or any subfield, such as macroeconomics, has its limits. No set of ideas is limitless in any meaningful sense. Yet, the notion of limits attaches itself naturally to macroeconomics in a way that would seem almost unnatural if applied to microeconomics. Modern macroeconomics is constantly reaching in one direction for its microeconomic roots and in the other direction for its policy relevance. Pronouncements by contemporary macrotheorists, together with the track record of contemporary policymakers, are enough to make us doubt that macroeconomics can reach roots and relevance at the same time.

Shiela Dow (1985, p. 225) suggests that a fundamental difference between new classicism, which she includes in her treatment of mainstream views, and post-Keynesianism, which she favors over the mainstream, is that the new classicists allow their micro-based theory to delimit their policy prescriptions while the post-Keynesians allow policy relevance to guide their theoretical formulations. If we accept Dow's characterization, we can take the gulf that separates these two schools as a measure of the difficulties faced by macroeconomists in search of a theory that is both theoretically sound and policy rich. And these difficulties can be multiplied in accordance with the title of a recent book by Edmund Phelps, *Seven Schools of Macroeconomic Thought* (1990), which suggests a divergence of views rather than a convergence in the field of macroeconomics.

Somewhere between microeconomic principles and macroeconomic phenomena lies a market process whose complexity imposes strict limits on macroeconomic theory—and even stricter limits on

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macroeconomic policy. The second section of my paper deals with this complexity by drawing on pre-Keynesian treatments of capital theory and suggesting that considerations of the economy's capital structure allow for the most forthright and insightful integration of the critical time element into macroeconomic theorizing. The third section argues that capital-free treatments of time in mainstream macroeconomics can be seen as indirect and inadequate ways of coping with the thorny issues of capital theory. The fourth section identifies the limits of macroeconomics in terms of entrepreneurial expectations in the context of a complex capital structure. Finally, the fifth section considers the nature of the limits of macroeconomics and the implications for policy activism, institutional reform, and theoretical advancement.

Capital Theory: It's About Time

Except for the Austrian school and some sectors of the Swedish and early neoclassical schools, the contending macroeconomic theories are united by a common omission. They neglect to deal with capital or, more pointedly, the economy's intertemporal capital structure in any straightforward and satisfactory way. Yet capital theory offers the richest and most promising forum for the treatment of the critical time element in macroeconomics. According to an early English view, capital puts a time interval between the beginning and end of enterprise (Jevons [1871] 1970, p. 226). In the Swedish construction, capital embodies the tying up of resources over time and is measured (in compound units of dollar years) by the waiting done by owners of capital (Cassel 1903, p. 96ff). The Austrians dealt with this same time element in terms of the "degree of roundaboutness" that characterizes the economy's production process (Böhm-Bawerk [1889] 1959, vol. 2, pp. 79–88 and *passim*). Each of these three formulations has served as a basis for theorizing about capital and could serve as a guide for devising a capital-based macroeconomics.

But capital considerations, to the extent they are accommodated at all in modern macroeconomics, are not well anchored in any of these early insights that link capital to time. In conventional income-expenditure analysis, enterprise has a beginning or an end, but not both. The existing structure of capital, summarily treated as the economy's capital stock, is taken as a given, an end of some process of accumulation whose beginnings are no part of the theory. Additions to the capital stock, investments, have only a beginning. Inspired by Keynes's animal spirits or by some other similarly unexplained

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state of business confidence or profit expectations, current investment activity must eventually come to some end, but that end comes into view only in theories of economic growth, not in macroeconomics per se. The relationships among macroeconomic magnitudes in the context of beginningless capital and endless investment do not adequately reflect the time element in macroeconomics.

Capital in mainstream macroeconomics is neither marked by beginning and end nor conceived as Casselian waiting. Theoretical constructions in which investment is simply one of several categories of spending do not allow for the two-dimensional measure suggested by Cassel. Spending by consumers, investors, and the government during a given accounting period is measured in dollars and not dollar years. And if net investment is to be added to the existing capital, then capital too must be measured simply in dollars. A time dimension cannot be accorded capital and investment without destroying their conformability with the other spending magnitudes. But if, in the same theoretical construction, the rate of interest is conceived as the price of capital, an internal inconsistency is introduced. Dimensional conformability requires that the interest rate must be the price of something measured in dollar years.[1] The common practice of glossing over such difficulties in capital theory by not explicitly assigning dimensions to capital was noted early on by Joan Robinson ([1953] 1971, p. 47). Flagging such dimensional difficulties here is not intended, as Robinson would have had it, as a prelude to a wholesale dismissal of neoclassical production theory but rather as further justification for maintaining an explicit time dimension in the concepts of capital and investment—and particularly in the treatment of the economy's capital structure as incorporated into macroeconomics.

The Austrian concept of "roundaboutness" is typically conceived as an average period of production and is expressed simply in years. It is the time dimension distilled from the dimensionally complex concept of waiting. If roundaboutness has been averaged over a total capital value, then there is a corresponding total roundaboutness that is conceptually equivalent to Casselian waiting. But roundaboutness, like waiting, plays no role in mainstream macroeconomics. It has been rejected on two grounds. One is an argument, initially made by John B. Clark (1924) and later defended by Frank Knight (1934), that in an ongoing economy production and consumption are, in effect, simultaneous. Accordingly, roundaboutness, if at all definable,

[1]For an application of dimensional analysis to the issue of the interest rate as a price, see Garrison (1988, pp. 49–51).

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is irrelevant. The other is the demonstration—implicit in the work of Piero Sraffa (1960) and explicit in a key article by Paul Samuelson ([1966] 1971)—that there is an inherent difficulty in ranking production processes either on the basis of capital intensity or on the basis of the (average) roundaboutness associated with each.

The first claim that production time is irrelevant lacks plausibility; the second claim that roundaboutness, which has both a value dimension and a time dimension, is uniquely related to neither of the two separate dimensions is not in doubt. Nor is it in doubt, for that matter, that the inverse relationship, emphasized by the Austrian economists, between roundaboutness and the interest rate is clouded by the *definitional* dependence of roundaboutness on capital value and hence on the rate of interest. But neither Knight nor Samuelson—nor anyone else—has provided adequate grounds for ignoring or downplaying the admittedly complex time element embodied in capital and investment.

The fact that conventional macroeconomics does not incorporate a capital structure or a time element in any fundamental way has telling consequences in terms of possible directions for development of macroeconomic theory and provides a basis for identifying and evaluating different schools of macroeconomic thought. If the economy's capital structure is not an integral part of the theoretical construction, then the market mechanisms that create and maintain that structure can be treated only in some summary of fragmentary fashion. A casual survey of macroeconomic literature suggests that there are two summary techniques and many of the fragmentary variety. The greatest contrast is exhibited between theories that assume these market mechanisms work perfectly well (and hence need not be analyzed) and theories that assume these markets mechanisms are totally dysfunctional (and hence cannot be analyzed). Representatives of the works-perfectly-well theories include much of monetarism and most of new classicism; representatives of the totally dysfunctional theories include fundamentalist Keynesianism as rooted in Keynes's 1937 restatement of his *General Theory* as well as Austro-Keynesian nihilism as exposited from the Keynesian side by G. L. S. Shackle (1974) and from the Austrian side by Ludwig M. Lachmann (1976).[2]

Theorists who do not incorporate a capital structure in their macroeconomics but reject either of the two extreme assumptions about the efficacy of the market mechanisms that create and maintain that

[2]For an extended capital-based contrast of Keynesianism and monetarism and a treatment of the Austrian alternative as a middle-ground position, see Garrison (1984, 1989).

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structure are left to pick and choose in ad hoc fashion. They must select from among many aspects of the market process some aspect of it that is thought to be particularly significant and worthy of attention. For Keynesianism as set out by income-expenditure analysis, expectations, which are explained largely by group psychology, dominate in determining the level of investment. The interest rate, which is governed largely by changes in liquidity preferences, plays a subordinate role in investment decisions, as reflected in an interest-inelastic demand for investment funds. For post-Keynesians, mark-up pricing made possible by oligopolistic elements in the economy satisfy the needs of the capitalist class for internal finance and enable investments to be undertaken. For new Keynesians, contracting costs for labor, which result in wage and price strickiness and in staggering of wage-rate adjustments, weigh heavily in translating parametric changes into changes in levels of employment and investment.

To single out aspects of the market process not explicitly related to the time element in the economy's capital structure is to overlook the intertemporally complex relationship between successive periods of investment and the resulting pattern of output. In theories with atemporal explanations of the level of investment in each period, the capital stock can be nothing other than a simple sum of the separate investment magnitudes. One notable theoretical development within new classicism, however, centers around an aspect of the market process explicitly related to the critical time element. Investment in each period consists partly of new investment and partly of continued investment, which reflects earlier—and possibly regrettable—investment decisions plus what is called time-to-build considerations (Kydland and Prescott 1982). Although the distinction between new and continued investment in the context of a theoretical construction that otherwise has no structure of capital is an ad hoc distinction, it is nonetheless one that is rooted—or at least could be rooted—in the most fundamental insights linking capital and time.[3]

[3]In view of their own critique of mainstream macroeconomics, the new classicists will not take lightly the charge of "ad hocness." But distinguishing between new and continuing investments has no theoretical basis in new classicism. Without a theory of capital structure, the distinction can mean only new or continued involvement of a particular firm. But for macroeconomic relevance, the distinction must apply to the economy's capital structure. The firm-based distinction and the structure-based distinction would be equivalent only if all production processes were characterized by complete vertical integration.

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Adjectival Time

How many different kinds of capital are there in a capital-using economy? Surely there are many, although most of macroeconomics pretends that there is only one. The capital stock grows monolithically as each year's investments are added to it. How many kinds of time are there in a capital-using economy? Surely there is only one kind of time, which underlies all macroeconomics, all economics, all social science, all science, and all reality as we know it. Nonetheless, a casual survey of modern macroeconomics literature reveals a proliferating taxonomy of time: calendar time, mechanical time, analytical time, Newtonian time, Bergsonian time, real time, historical time, expectational time, market processes that are "in time," and market processes that are "out of time."

The long list of adjectives that have been appended to time partly reflects both a variation in writing style and a lack of standardized terminology. Admittedly, several contrasting pairs can be gleaned from the list to express the same—or very similar—meaning. For macroeconomists who think in terms of the economy's capital structure but who read a macroeconomics literature free of such considerations, adjectival time takes on a special meaning. The taxonomy of time is a surrogate, however cryptic and otherwise inadequate, for a taxonomy of capital; it sheds only the most indirect light on the market processes that maintain and modify the economy's intertemporal capital structure.

There are two analytically separable—though actually interrelated—sets of issues for which kinds of time are proxies for other, more substantive aspects of a capital-using economy. One set of issues, which turns on the distinction between historical and analytical time, has to do with the nature of the economy's capital structure; the other set of issues, which makes use of expectational time, has to do with entrepreneurial decisions that lead to the maintenance or modification of the capital structure.

Historical time , which is characterized by an essential irreversibility, is commonly contrasted to analytical time, which, purportedly, can run in both directions. Movements in analytical time are analogous to movements in space. In analytical time, eggs can be cracked and then uncracked; volcanoes can erupt and then unerupt; investment projects can be commenced and then uncommenced. In historical time, some things—cracked eggs, erupted volcanoes, and committed capital among them—cannot be undone. While the notion of time running in both directions, even as a thought experiment, will not survive critical contemplation (How about all six directions? At

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the same time?), the issues addressed can be given more direct—and therefore less cryptic—expression by considering the nature of the economy's intertemporal capital structure.

Capital goods that make up the structure differ in terms of durability and specificity. Relationships among the heterogeneous collection of capital goods vary in degrees of both atemporal and intertemporal substitutability and complementarity. If capital goods were wholly nonspecific, if the collection of them were fully homogeneous so that any one capital good is a perfect substitute for any other, then production processes could proceed as if time ran both ways. A half-finished performance hall could be completed—with no effects on cost or construction time—as a bowling alley; the production process that yields musical instruments could—with an eleventh-hour change of mind—yield bowling pins and bowling balls instead. It would be as if the construction of a performance hall and the making of musical instruments, once commenced, were then uncommenced to facilitate the construction of a bowling alley and the production of bowling equipment: The total production time—into which the backward-moving components of time enter negatively—would be no greater than if bowling had given shape to the production process from the start.

Treatments of macroeconomic issues in which historical time is played off against analytical time are capital-based macroeconomics in silhouette. To feature time irreversibility is to recognize that the intertemporal capital structure has a particular profile. But the black-and-white distinction between historical and analytical time is too crude and too cryptic to shed any light on the actual complexity of the capital structure and on the causes and consequences of capital maintenance and capital restructuring.[4]

Expectational time is a phrase that refers to a time horizon rather than to the actual passage of time in one direction or another. For what period of time do investors plan? The formation of expectations and the commitment to investment undertakings have a critical time dimension in macroeconomics. But the acts of forming expectations and making commitments are largely unanchored in mainstream macroeconomic theory; they lack both subject and direct object. The subject of such acts is the entrepreneur, who has a shadowy existence in most of mainstream economics: in microeconomics as well as

[4]It could be argued that the distinction made here and in subsequent sections between maintenance and restructuring is itself too crude. The intent is to include as maintenance all sorts of market-specific adjustments that macroeconomics can safely ignore and to include in restructuring the systematic shifting of resources within the structure of production that alters the intertemporal pattern of output.

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macroeconomics. The proximate objects of such acts are the components of the capital structure; any contemplated additions or modifications are evaluated in light of the capital structure as it is currently perceived to exist.

The attempt to finesse a theory of entrepreneurship in the context of a complex intertemporal capital structure by incorporating expectational time into macroeconomics has had but little success. Polar contrasts can be made (as in Keynes [1936] 1964, chap. 5) between short-run expectations (which are formed and reformed continuously and confidently on the basis of timely and relevant feedback) and long-run expectations (which are totally baseless acts of imagination). And plausible claims can be made about changes in planning horizons attributable, say, to a change in the rate of technological progress or to an increase or decrease in stability of a policy regime. But identifying the macroeconomic significance of changing expectations that cause investment activity to turn from maintenance of a given capital structure to structural modification requires more than considerations of expectational time can deliver. Explicit consideration must be given to the capital structure itself and to the entrepreneurs who form expectations on the basis of their perception of it.

Does It Matter That Expectations Are Subjective?

Reflective writings on economic theory acknowledge the essential subjective character of its fundamental concepts. Consumption utilities, even costs, and certainly entrepreneurial expectations are subjective in ways that few economists would dispute. For issues of basic methodology and philosophy of science, subjectivist considerations are given great weight. And in modern macroeconomics, schools of thought can be categorized on the basis of whether or not—and to what extent—they give play to the subjectivity of expectations.

The polar extremes identified earlier, Keynesian fundamentalism and new classicism, can serve to illustrate. In the first extreme, critical questions about the state of long-term expectations, about the profitability of investment activities, guide discussion about the future of the macroeconomy. However, answering such questions with Keynes's oft-quoted punchline that "We simply don't know" cuts discussion short. In the second extreme, it is postulated that entrepreneurs, if not economists, *do* know or that they behave, collectively, as if they do. With the assumption of rational expectations, the essential subjectivity of entrepreneurial expectations is downplayed if not completely eliminated, as is the distinction between the past and the future.

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Identifying the limits of macroeconomics requires that we reject both polar extremes. Rather than make some assumption that either features or hides the subjectivity of expectations, we must seek a substantive answer to the question "Does it matter that expectations are subjective?" Considerations of the nature of entrepreneurial activities in the context of a complex intertemporal capital structure suggest a hedged answer: "Sometimes it doesn't, and sometimes it does." In the most general terms, our specific answer depends on whether the intertemporal structure of capital is simply being maintained or is undergoing systematic modifications in the face of some economywide change in market conditions.

Karen Vaughn (1980) has offered and defended a substantive answer to the similar and possibly more fundamental question "Does it matter that costs are subjective?" As a preliminary to policy prescription, this question about costs in the economically relevant sense of the subjective valuation of foregone opportunities becomes "Do observable market prices accurately reflect inherently unobservable costs?" Vaughn argues that they do but only under conditions of full equilibrium. She then juxtaposes this conclusion with the more common belief that policy intervention finds its greatest justification in conditions of persistent disequilibrium (Vaughn 1980, p. 710f).

Costs, the consideration of which underlie policy prescription, can be objectively measured only when there is no need for policy. This is the essence of the conundrum identified by Vaughn. A similar conundrum, conceived possibly as a corollary to the subjective-cost conundrum, identifies the limits of macroeconomics in terms of subjective expectations. Entrepreneurial expectations about the future can be surmised from entrepreneurial activity of the immediate past only in the instance in which entrepreneurs are pressing ahead to complete the projects that they have already initiated. Such a situation might occur if, under unchanged market conditions, market forces have been working and continue to work to maintain an intertemporal equilibrium. That is, only when entrepreneurial expectations about the future can be accurately represented as a projection of recent and ongoing entrepreneurial activity does the essential subjectivity of expectations not matter. Thus, the macroeconomic theorist can be confident that considerations of subjective expectations pose no difficulty for his theory only in circumstances in which there is no need for macroeconomic policy.

In circumstances of systematic intertemporal discoordination, however, considerations of expectations must dominate macroeconomic theorizing. Suppose that near the end of a period of economic

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expansion, it becomes clear to entrepreneurs—and even to economists—that market conditions favorable to continued expansion are unlikely to prevail. For the argument at hand, it does not much matter whether the economy is in the final throes of a demand-driven boom or on the eve of some dramatic but only vaguely anticipated change in supply conditions. In either case, macroeconomic theory has to deal with the fact that entrepreneurs are no longer pressing ahead but are instead adapting to change. And entrepreneurs' adaptations are guided by their expectations about the new market conditions and about how other entrepreneurs are likely to adapt.[5]

In a period of macroeconomic disequilibrium, when actions in the future are not simple extrapolations of actions in the recent past, all the thorny issues of capital theory come into play. What is the best thing to do with a half-completed performance hall in light of the changing market condition? Some such projects will be completed almost as initially planned; others will be completely liquidated. Some will be modified in various degrees in terms of the resources and techniques used; others will be altered to some small or great extent in terms of the ultimate objective. Thus, considerations of capital specificity and durability and of atemporal and intertemporal substitutability and complementarity among capital goods and between capital goods and other resources, as perceived subjectively by each entrepreneur, all come into play (Lachmann [1956] 1978).

What I have offered as a subjective-expectations corollary to Vaughn's subjective-costs conundrum can itself be expressed in terms of subjective costs. Under conditions of full competitive equilibrium, costs at the margin are adequately measured by observable market magnitudes. The entrepreneur borrows funds at the market rate of interest and undertakes projects that are just worthwhile. What is popularly called the "cost of capital" refers to both the rate of interest and the rate of return for the entrepreneurs whose activities maintain the marginal conditions. But it is precisely these marginal relationships that are nullified by an economywide disturbance. Macroeconomic disequilibrium drives a wedge between the rate of interest and the newly formed expectations about the rate of

[5]In application, the distinction between "pressing ahead" and "adapting to change" parallels the distinction between maintenance and restructuring as spelled out in footnote 4. Also, it should be noted that Vaughn's subjective-cost conundrum turns on the distinction between full equilibrium, which exists only in the textbooks, and disequilibrium, which characterizes market processes as we know them. My subjective-expectations corollary turns on the distinction between actual market processes as they function under normal conditions and those same market processes as they function in the face of some economywide disturbance.

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return on projects that were initiated before the disequilibrating disturbance. When capital maintenance turns to capital restructuring, the activities of entrepreneurs can no longer be explained in terms of marginal conditions and observable interest rates: It is a poor entrepreneur whose next best alternative is the bank rate of interest.[6]

Macroeconomic theory that translates changes in market conditions into movements in macroeconomic variables must hinge critically on the actual and perceived relationships that characterize the economy's intertemporal capital structure. In macroeconomic disequilibrium, it matters greatly that entrepreneurial expectations are subjective. The theory, in fact, is no better than its treatment of expectations. Thus, in the very circumstance of some economywide disturbance Vaughn's conundrum looms large: The greater the perceived need for stabilization policy, the more tenuous the theory on which such policy is based.

Further, by implementing policy either on the false premise that the subjectivity of expectation does not matter or with the realization that it does—somehow—matter, policymakers have the effect of making the theory even more tenuous. This compounding effect, which involves the interplay between theory and policy, will be recognized as central to the Lucas critique and to Newcomb's decision problem as applied to policy activism. However, the conundrum highlighted here is independent of, and logically prior to, those identified by Lucas and Newcomb.[7]

Demand-Siders, Supply-Siders, Both and Neither

Attention to the economy's intertemporal capital structure virtually precludes dealing with macroeconomic problems and formulating macroeconomic policy in terms of aggregates that summarily relate the demand side of the economy to the supply side. Intertemporal discoordination upsets supply-and-demand relationships within the capital structure, so that the time-consuming process that transforms the economy's basic resources into products that satisfy consumer

[6]A key difference between growth theory and macroeconomic theory can be stated in terms of the interest rate's serviceability as a measure of the relevant opportunity cost. The centrality of the interest rate in growth theories, which typically concern themselves with the economy's equilibrium growth path, is not in doubt; the significance of the interest rate in macroeconomics, which typically deals with circumstances in which marginal conditions have been upset on an economywide basis (e.g., by a collapsing demand-driven boom or by a supply shock) is a matter of some controversy.

[7]The Lucas (1981) critique is based on the market participants' ability to anticipate policy; Newcomb's decision problem, as applied by Frydman et al. (1982), arises when market participants and policymakers try to anticipate one another.

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demand is characterized by a complex pattern of excess supplies and excess demands. The problem of identifying and dealing with the specific and interrelated excesses is, in the final analysis, an entrepreneurial problem. The solution to the problem requires some systematic tendency for entrepreneurial expectations to be correct; hence, the solution requires institutional arrangements that automatically reward correct expectations.

Macroeconomic debate over the past several decades has been dominated by Keynesian policy activists, who advocate demand-management policies; by their monetarist critics; and by supply-siders, who have attempted to shift the focus of policy from the demand side of the economy to the supply side. Each side of the market has served as the basis for effective political rhetoric, as exemplified by the demand-oriented Democratic administrations of the 1960s and the supply-oriented Republican administration of the 1980s. Pointing to periods during which such one-sided policies for managing the macroeconomy were actually pursued effectively or even consistently is, of course, another matter.

Adding capital theory to macroeconomics as the most direct and explicit way of accommodating the critical time element in economic relationships warns against exclusive focus on either side of the market. Considerations of expectations and the critical role of the entrepreneur suggest that there may be an unbridgeable gap between macroeconomic theory and activist policy. The conundrums inherent in capital-based macroeconomics imply a balance in terms of sides of the market and a contrast between theory and policy: On analytical issues, macroeconomists should think of themselves as "both-siders"; on policy issues, as "neither-siders." Macroeconomic theory can shed light on the nature of the problems that entrepreneurs face in conditions of economywide disequilibrium; policy activism can do little to help solve what in essence is an entrepreneurial problem.

Spelling out the limits of macroeconomics in terms of capital structure and entrepreneurial expectations is not intended to imply that macroeconomics should be abandoned entirely in favor of microeconomics. Here too the intended implications turn on the distinction between theory and policy. For policy, clearer recognition of the limits of macroeconomics strengthens the case for rules over discretion. It argues for the abandonment of policy activism in favor of institutional reform. If policy activism is unlikely to get the economy out of macroeconomic difficulties, institutional reform that, among other things, eliminates the threat of policy activism may well keep the economy from getting into macroeconomic difficulties.

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For theory, the explicit detailing of the limits of macroeconomics points to possible margins where theory might be improved. More pointedly, translating macroeconomic ideas, which are expressed in the language of adjectival time, into the macroeconomics of capital structure may have an especially high payoff. And paying attention to the particular kinds of problems that entrepreneurs face during periods of economywide capital restructuring may provide the soundest basis for reforming the economy's macroeconomic institutions.

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The Case for Zero Inflation

Stephen L. Neal

Benefits of Zero Inflation

The Federal Reserve System (the Fed), a creation of Congress, has the ability to end inflation. If Congress will pass legislation to establish zero inflation (or what economists call "price stability"[1]) as our nation's policy and the Fed will implement it, we will soon enjoy long-term government interest rates of 3 to 4 percent (short rates even lower), mortgage interest rates of about 6 percent, and business and consumer loans at around half of today's rates.

By eliminating inflation, we will establish the essential foundation for the highest sustainable levels of economic growth, employment, savings, capital formation, investment, productivity, competitiveness, and efficiency. We will greatly reduce the likelihood of future recessions, make our country more competitive in world trade, and improve the standard of living for everyone. All of our people will benefit; no one will lose.

Price Stability and the Deficit

Our economy will perform even better if we accompany zero inflation with zero budget deficits. House Joint Resolution 24, which I have sponsored, would achieve price stability and reduce the budget deficit more—and more quickly and painlessly—than any other way.

We reduce the budget deficit this way: As soon as our government is seen as serious about ending inflation, the "inflation premium" in interest rates will begin to disappear and long-term rates will start to fall. Since our government is paying interest on more than $3 trillion of national debt, each 1 percent drop in the average interest

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[1]Zero inflation and price stability both mean a level of inflation so low that it is inconsequential to decisionmaking.

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rates paid will cut the deficit by $30 billion dollars a year. Each 1 percent drop in average interest rates saves $30 billion a year in interest—year after year—permanently.

As we continue reducing the deficit by lowering interest rates, and lowering interest rates by reducing inflation, the benefits compound. We will have permanently saved hundreds of billions of dollars during the five years that it will take to achieve zero inflation.

Savings, Investment, and Trade

When the American people understand that the value of their savings will not be destroyed by inflation, they will save more. Increased personal savings, plus savings generated by cutting the deficit, will generate a massive new pool of savings that will create the capital economists say is essential for America's future. In short, more savings means more investment. More investment means more jobs and a more productive and competitive economy, both at home and in world trade.

If, after achieving zero inflation, we maintain it, the benefits will be permanent. Working people, low-income people, and retired people will not have to live with the constant specter of prices for food, medicine, education, clothes, energy, and transportation rising year after year, usually faster than their incomes. They will not see their retirement savings eaten away by inflation.[2] And when they borrow, they can count on low interest rates.

Young families and others will be able to buy homes. At 11 percent interest, a 30-year $80,000 mortgage costs $762 a month. At 6 percent, that mortgage shrinks to $480 a month. At the lower rate, more working families could qualify for home loans and would astoundingly save more than $100,000 (including interest) during the life of the loan. As inflation comes down, mortgage rates will drop, the housing industry will boom, more people will be at work, and we can at last provide affordable housing. Also, small businesses and farms, often fighting for survival and always in debt, can flourish with stable prices and low-cost capital.

Our major international competitors, Japan and West Germany, will not tolerate high inflation. As a result, their prices are stable and predictable, and their costs of capital are low. Zero inflation and lower interest rates will make us more competitive in world trade. We can help reclaim our leadership position in the world economy and provide more jobs for our people. The benefits will be fabulous for all aspects of our economy and for all Americans.

[2]At a 5 percent annual rate of inflation money loses half its value in just 14 years.

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Costs of Achieving Zero Inflation

But what about the costs? Clearly, if we were to try to accomplish zero inflation overnight, the result would be a major recession. But I am recommending that we get there over a period of years, not overnight.

Two factors clearly indicate we can reach zero inflation without much pain. First, we are now operating in a period of labor shortages. Even with moderate growth, most people who want work can and will be able to find it.

Second, as inflation decreases, long-term rates will come down, enabling us to continue moderate growth even as we coax inflation out of the system. Thus, the cost of achieving price stability would be an economy growing about as it does now for a relatively short period—not booming, maybe, but not bad, given today's rate of inflation.

Importance of Credibility

The current recession should not last long if government will provide coherent policy. We should insist on a time frame for achieving zero inflation that will not cause a major recession, which would inflict unnecessary pain and sap our will to succeed. I think five years is about right, but if we can achieve our goal a little sooner or if it takes a little longer, that difference is not critical. What is essential is that our plan be credible.

Misconceptions about Inflation

A few other questions should also be explored. Some people think inflation is caused by budget deficits. But if so, how were we able to bring inflation from double digits to 4 percent in the early 1980s while we were doubling the national debt? How could inflation have stayed under 5 percent while fiscal policy tripled the debt by 1989?

Some people think that inflation is good for employment. If true, why is it that Japan, West Germany, and other low-inflation countries have high employment while high-inflation countries like many in Latin America experience low employment? And why was it that the high inflation of the 1970s in our country was also a period of very high unemployment, so-called stagflation? Employment created by inflation is temporary. Because most recessions (and the resulting unemployment) are created to stop inflation, inflation cannot be good for long-term employment. If there is no inflation, there is no need for recession.

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Some people think inflation is caused by supply-side shocks to oil and commodity prices. If so, how do you explain the low inflation in West Germany, Japan, Switzerland, and other countries that experienced even worse oil shocks than did the United States?

Some people want to exert more political control over the Fed by adding the Treasury secretary to the Fed's board. Legislation has been introduced in Congress to politicize the Fed. But would not that change almost ensure stimulative policies during election years and contractions during the years after? Some people say the Fed should be able to do whatever it wishes and not be accountable to anyone. But the Fed is part of our government; its actions have an enormous impact on all of our lives. So should it not be held accountable for producing the policy that is best for our country?

Some people think that inflation is caused by factors beyond our control and that there is nothing we can do about it. But inflation still is, as the dictionary says, too much money chasing too few goods. And the Fed controls the supply of money. The Fed wants to end inflation and is trying, but because so many in the Congress, the administration, and the public want the Fed to do so many different and inconsistent things, nobody believes the Fed. Passage of legislation requiring zero inflation will make our commitment clear and effective.

Conclusion

The Subcommittee on Domestic Monetary Policy, which I chair, has been exploring these issues and others in public hearings. The Federal Reserve Chairman, Alan Greenspan, and all the regional Federal Reserve bank presidents have endorsed my legislation. It has also been endorsed by two Nobel Prize–winning economists, by numerous other economists, by the U.S. Chamber of Commerce, and by the American Farm Bureau Federation, among others.

We hope support will be widespread. We think members of both major political parties should like the idea. Democrats will be especially attracted by the great benefits to our traditional constituency of working people, older Americans, farmers, small businesspeople, and low-income families. They are the ones who suffer most from rising prices and are least able to protect themselves. They generally are borrowers, not lenders, and would especially benefit from low interest rates. Republicans should like zero inflation because nothing is better for the stock market than low interest rates. Since inflation has the same effect on purchasing power as a tax, reducing inflation from 3 percent to zero is like lowering taxes by 3 percent. And

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all lovers of freedom, our nation's great treasure, should like zero inflation, regardless of their political stance.

There is some cost in fighting inflation, but the costs of not achieving zero inflation are immense—hundreds of billions of dollars of unnecessary government interest payments, constantly rising prices, less permanent growth and job creation, low savings, and a less-efficient and less-competitive economy.

We know the benefits of achieving zero inflation are great and the costs are small, and we know how to do it. We can help cure the major disease of our time—short-term thinking leading to short-term policies—by continuing our drive toward zero inflation. Adopting zero inflation as our nation's policy would be a tremendous gift to the American people—a gift that would "keep on giving" forever.

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Commodity Prices and Monetary Policy: What Have We Learned?

Wayne D. Angell

Money, macroeconomics, and forecasting cover the important ingredients in any policymaking strategy. While I will briefly comment on what I believe is the current state of monetary policymaking, my key focus will be on the role of forward-looking market price indicators—such as commodity prices—in any monetary policy strategy in our current environment. More specifically, I will discuss how I believe these indicators have been helpful and why they can contribute to a more successful monetary policy.

Because some key reasons for using forward-looking commodity price indicators are often overlooked by academic researchers, I want to outline the rationale for using these indicators and review some recent empirical evidence pertaining to their usefulness. Later in this paper, I will discuss how commodity prices have actually been used in implementing monetary policy in recent years.

The Current State of Monetary Policymaking

In recent decades, we have witnessed the unreliability of several strategies for monetary policymaking. We have learned a good deal about what types of monetary policy strategies no longer work the way theory would have us believe. Most monetary economists now recognize that levels of nominal and real interest rates, real economic variables such as employment, trends and rates of economic growth, and even narrow monetary aggregates are no longer reliable as the sole guides or targets for monetary policy. While some of these variables were deemed theoretically inappropriate, others have been

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rendered ineffective, possibly because of deregulation, global integration of financial markets, and revolutions in computer and information-processing technology.

Despite these negative lessons—discovering what does not work—we have also learned some positive lessons during this same period. In recent years, we have seen an emerging consensus that the proper goal of monetary policy is price stability. It may well be that before price-level benefits are apparent, support for this goal will soften as the negative effects of an economic slowdown are experienced. It is propitious, therefore, to point out that the apparent employment and price tradeoffs can be mitigated by timely use of forward-looking price indicators.

Even economists who generally agree on the goal of price stability, however, may disagree on exactly how to achieve it. Specifically, economists disagree as to what procedures, what instruments, and what policy guides are best suited to achieve price stability.

As they ponder possible alternatives, the first task of monetary policymakers is to be able to accurately gauge the current posture of monetary policy. Policymakers must be able to judge whether policy is "tight" or "easy," whether the current stance of policy is inflationary or deflationary.

At one time, I had a great deal of confidence in monetary aggregates as reliable targets or guides to monetary policy. After all, in the 1960s and 1970s much evidence was mustered suggesting that monetary aggregates did reliably signal the "tightness" or "easiness" of monetary policy better than did the level of interest rates. And in certain environments, the monetary aggregates are certainly very useful policy guides.

In the early 1980s, however, it became obvious that narrowly defined monetary aggregates no longer accurately gauged the stance of policy. Shortly after this deterioration, I began to consider what information could help to interpret movements in the monetary aggregates or, rather, what type of indicators could be used to supplement monetary aggregates in gauging the posture of monetary policy.

Over the years, I have become convinced that information from forward-looking financial market indicators can usefully serve this supplemental role. Several types of market price information can aid in this task. For example, commodity prices, foreign exchange rates, and bond prices have all been proposed and used for this purpose. For reasons of brevity, however, I will focus my attention primarily on commodity prices or indices of commodity prices.

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Some Reasons Why Commodity Prices Are Useful Monetary Policy Indicators

There are many reasons why commodity prices may provide useful information or may serve as useful indicators for monetary policy. First, the data-measuring commodity prices have a number of advantages over the type of data-measuring quantity variables such GNP statistics, domestic output and income statistics, or statistics pertaining to the monetary aggregates. Commodity prices, for example, are readily available, are observed at frequent intervals, and are not subject to revision and adjustment as are the above-cited quantity data. Moreover, commodity prices have communication advantages in that they are simple and easy to understand. While individual commodities are sometimes affected by special factors, the use of broad-based indices of commodity prices minimizes the probability that movements in such indices reflect single-commodity supply factors.

Second, commodity prices are more flexible than other categories of prices. If commodity prices are flexible while other prices remain somewhat sticky, then changes in monetary policy may consistently have an impact on commodity prices before affecting other prices; commodity prices may yield timelier signals about policy change than do other prices. This characteristic may explain why commodity prices serve as useful leading indicators of inflation.

Third, commodity prices are forward looking. They are determined in auction markets and behave similarly to the prices of bonds, equities, or foreign exchange. Accordingly, commodity prices incorporate agents' anticipations of fundamental market forces as well as anticipations of both policy change and movements in general prices. In short, commodity prices embody expectations of the future.

Fourth, commodity prices may also serve as useful indicators for monetary policy because they enter the production process at early stages of production. Because of these facts, changes in commodity prices may tend to lead movements in broader price indices such as producer or consumer price indices.

In addition to these useful indicator properties that are pertinent for domestic monetary policy, commodity prices can be a useful ingredient in the international coordination of monetary policy. For example, when central banks monitor movements in commodity prices in conjunction with movements in exchange rates, they can often determine whether a given inflation is local or global in character. This information may provide guidance as to which country should pursue a relatively tighter policy and which should pursue a relatively easier policy.

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If a domestic currency is depreciating against a basket of other currencies while broad indices of commodity prices are weak, then perhaps domestic monetary policy should ease relative to foreign monetary policy. Market price indicators, therefore, may help to signal the type of policies that should be undertaken in different countries and help to coordinate monetary policy.

Coordination of monetary policy that stabilizes exchange rates, however, also muffles information previously provided by exchange rate movements. In this situation, the information provided by commodity price movements becomes increasingly important. In particular, commodity price movements can provide vital information as to the monetary policies of those countries that coordinate policy; commodity prices also can provide valuable information as to global inflation or deflation and thereby can help anchor the international monetary system. In this context, it may be especially important for a key currency country to monitor commodity prices in assessing international price developments and the condition of the international economy. It is noteworthy that in 1987, then–Treasury Secretary James Baker proposed that commodity prices be included among the set of indicators monitored by the G-7 countries.

Thus, there are many practical and theoretical reasons why broad indices of commodity prices may serve as useful indicators for monetary policy, particularly if they supplement or complement information from monetary aggregates. While some uses of commodity prices as a policy guide are occasionally overlooked in academic literature, commodity prices may provide very useful information to the practical monetary policymaker.

Evidence Pertaining to the Policy Usefulness of Commodity Prices

Of course, presenting practical and theoretical rationale for the use of commodity prices as monetary policy indicators is fine. But empirical evidence supporting this rationale is also essential. Empirical evidence pertinent to using commodity prices as policy guides takes two general forms: first, the relationship between changes in monetary policy and changes in commodity prices, and second, the relationship between changes in commodity prices and changes in general prices.[1]

Recently, a good deal of empirical evidence has been produced on these very issues. I cannot hope to survey all of this evidence,

[1]See Angell (9189), Brayton et al. (1993), and Reinhart (1991).

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but I will present a very brief summary. First, existing empirical evidence on the relationship between changes in monetary policy and changes in commodity prices comes from a number of bodies of literature. Specifically, the following all yield information about this issue: formal empirical evidence of the effects of changes in monetary policy on broad indices of commodity prices, historical studies of these effects, and studies of the key determinants of commodity prices. In fact, the overall evidence is mixed. But much evidence does suggest that changes in monetary policy do significantly influence commodity prices.

Second, evidence examining the relationship between changes in commodity prices and changes in general prices or inflation also takes several forms. For example, turning point or leading indicator evidence and evidence relating to "causality" or predictability both provide information about this relationship. Turning point evidence indicates that commodity prices are useful leading indicators of movements in general prices. More specifically, this evidence suggests that changes in commodity price movements usually precede movements in various measures of general prices. A number of studies corroborate these findings. False signals, however, do occur; although commodity prices are not perfect leading indicators of inflation, they are useful enough to be included in a well-known index of leading indicators of inflation.

A number of empirical studies indicate that commodity price movements do significantly "cause" or predict general price movements. But these studies generally find that the incremental information content provided by commodity prices—beyond that contained in a host of other variables—does not appear to be great. Nonetheless, several studies suggest that the importance of commodity prices may be increasing.

In sum, the empirical evidence suggests that (1) changes in monetary policy influence commodity prices, and (2) movements in commodity prices both lead and "cause" changes in general prices. To be sure, this evidence also indicates that commodity prices are not perfect monetary indicators and should be neither the sole indicator nor the target for monetary policy. Nonetheless, most economists investigating this issue do agree that commodity prices yield useful information for perceptive monetary policymakers and, accordingly, can be useful supplementary indicators to the monetary aggregates.

Recent Experience

In recent years, several important examples have demonstrated how commodity prices can be used to improve monetary policy.

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These examples suggest that commodity prices improved monetary policymaking when they were used, and probably would have improved monetary policymaking at other times if they had not been ignored.

During the period from spring to autumn 1989, broad indices of commodity prices were trending down; the level of commodity prices was actually declining. World commodity prices were also quite sluggish. Therefore, commodity prices were signaling that an easier policy stance was appropriate and that the monetary restraint implemented in earlier months had taken root.

At the same time, other key market price indicators were providing corroborating evidence. The spread between the federal funds rate and the 30-year Treasury bond rate became progressively more inverted during this period. The bond rate itself persistently declined from March until autumn, and the dollar was appreciating. Market price indicators all suggested that policy ease was in order.

The federal funds rate was, in fact, reduced during this period, thereby ameliorating what could have been an even more abrupt slowdown in economic activity. If this action had not been taken, it is likely that the foreign exchange rate would have further appreciated, thereby contributing to a more pronounced plateau in net exports. Thus, for the first time, the Federal Reserve used commodity prices effectively to improve the conduct of monetary policy.

In late 1989, however, events had changed. In particular, by December 1989 commodity prices had stabilized. Commodity prices were signaling that additional policy ease would be misinterpreted and could lead to higher long-term interest rates. Furthermore, the dollar had begun to depreciate, and long-term bond rates had stabilized so that the federal funds rate had fallen relative to the long bond rate. Market prices were signaling that additional ease was not appropriate at that time.

At the Federal Open Market Committee (FOMC) meeting in December 1989, I dissented from the easing action undertaken. In dissenting, I explicitly stated for the record:

> Policy decisions should rely mainly on leading indicators, including commodity prices, the exchange rate, the yield curve, and money supply growth. Attention to such indicators had served policy well in the past. . . . At this meeting, price-level indicators were not signaling a need for further ease. In these circumstances, an additional drop in the federal funds rate, coming after two previous easing moves in the fourth quarter, could raise doubts about the System's commitment to its objective of price stability, . . . and drive

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up long-term interest rates, thereby increasing the likelihood of economic instability.[2]

As it happened, the Federal Reserve went about seven months without further easing action and by that time circumstances had changed. For me, the lesson of this particular episode is that commodity prices can help in bringing about the better timing of policy action: Commodity prices can help the monetary policymaker to take the correct action earlier.

In addition to these earlier episodes, oil-price fluctuations have also bolstered the case for monitoring commodity prices. In situations when the price of a key commodity like oil dramatically changes, it is particularly useful to monitor those commodity prices not influenced by changes in energy prices. In other words, it is useful to continue to monitor those commodity prices still reflecting the influences of monetary policy.

Broad indices of non-oil commodity prices have provided very useful information to monetary policymakers. In particular, at the time of the Persion Gulf War, non-oil commodity price indices clearly signaled that monetary policy was not accommodating the oil price increase. These non-oil commodity price indices, therefore, were signaling that monetary policy was maintaining an anti-inflation policy posture. The persistent decline in non-oil commodity prices together with declines in bond yields indicated that the January 1991 cut in the discount rate was fully justified.

Conclusion

In recent decades we have witnessed unreliable performance of some one-time key indicators of monetary policy. Nonetheless, we have learned important lessons about what can work, and we have reached a consensus that price stability is the proper goal of monetary policy. Many reasons and a good bit of empirical evidence support the use of commodity prices as a monetary policy indicator that is useful in pursuing this goal. Practical experience continues to suggest that commodity prices yield beneficial information and are quite helpful in the policymaking process.

In addition to being theoretically sound and practically useful, commodity prices may also serve to help us understand important monetary episodes of the past. A number of studies of this sort are on my personal research agenda.

[2]Board of Governors of the Federal Reserve (1990, p. 238).

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Market Constraints on Central Bank Policy

Bruce Kovner

Although I cannot speak as an economist, I can perhaps offer some comments from the perspective of a trader. Having spent nearly every day for the past 15 years evaluating markets and trading in them, I have been part of the process that has transformed world capital markets. This transformation is worth discussing for a moment or two before going on to some observations about market pricing and constraints on monetary policy.

Emergence of Global Financial Markets

In the years immediately following the abandonment of Bretton Woods, there were few sophisticated traders of foreign exchange, and even fewer who simultaneously monitored the real yields available on government instruments around the world. And even if there had been such traders, transactions would have been extraordinarily difficult to carry out. There were no futures contracts on foreign fixed-income instruments; arbitrage in the cash markets was virtually precluded by regulation and practice; and information on monetary and macroeconomic phenomena was difficult to acquire quickly and systemically, plus most market participants were not prepared to do so in any case. In contrast, today 200,852 Reuters screens are used worldwide by thousands of market participants who analyze international monetary phenomena on a continuous, 24-hour real-time basis. We can monitor futures contracts on German short-term and long-term interest rates, French rates, British rates, Japanese rates, Australian rates, New Zealand rates, Canadian rates, and even Spanish rates. U.S. interest rates trade 24 hours a day throughout

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The author is Chairman of Caxton Corporation. This paper is based on his luncheon address at the Cato Institute's Ninth Annual Monetary Conference.

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the world. Equities index contracts are traded in the United States, the United Kingdom, Germany, France, Japan, Switzerland, and elsewhere.

The growth of a class of market participants, capable of evaluating and responding quickly, has been even more important. In 1975, few traders would have known how to evaluate German or Japanese monetary operations, for example, even if those traders had the technical means to observe them. Today, if the Bundesbank sneezes, the international trading community is ready with "gesundheit." We sit at our screens 24 hours a day (collectively) watching for announcements about the size of open-market operations in Japan or about the level of reserves in the system, and we look at every German repurchase for signs of policy changes. Every move of the central bank or of treasury officials in a major world economy is scrutinized with care and some measure of expertise.

Rapidity of Price Discovery

The development of this human capital, this ability of capital markets to process and evaluate monetary policies, has altered the character of price discovery, making it more rapid and more decisive. Let me offer two examples from the U.S. market. I remember the mid- and late-1970s in the United States when the 30-year bond just refused to go down despite months, even years, of negative real interest rates. As a trader, I remember trying regularly to sell the U.S. bond week after week as inflation numbers and monetary aggregates were published suggesting that U.S. inflation was bad and getting worse. Yet yields refused to budge over 8 percent. Bond buyers would emerge on every dip in prices. They remained confident that inflation could not be as bad as it looked, despite the consistent trend of bad inflation numbers. Only after two years of solid buying of bonds at these negative real interest rates did bond yields finally take off, reaching, as we know, 14 percent by 1981.

The experience of the United States during 1989 and 1990, on the other hand, was surely no repeat of the pattern of the 1970s. In 1989 and 1990, the Fed's ease in short-term rates was no guarantee of lower long bond yields. On the contrary, each monetary ease was preceded and followed by an extensive debate on whether the Fed was moving prematurely to ease—conditional on views about prospective inflation. On two occasions, Fed ease was followed by significantly higher yields in the long bond. After the Fed ease of December 20, 1989, long bond yields rose 15 basis points within the week and 120 basis points within four months (from 7.81 percent

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to 9 percent). Simultaneously, the dollar dropped five pfennigs against the Deutsche mark in the week following that ease. The Fed easing was not resumed until long bond yields had retreated from their 9 percent peak and the dollar had stabilized—a period of seven months. The next Fed ease was also greeted with skepticism by the market. Yields on long bonds rose by 10 basis points after the Fed ease of 25 basis points on July 13, 1990.

Significance of the New Market Discipline on Fed Policy

I must offer a few words of caution about the significance of the new market discipline on the conduct of monetary policy. The idea that markets "know everything"—that policymakers can base their decisions on what the markets "know"—is a fallacy. It is true that markets have become more efficient at reflecting quickly in pricing any perceived consequences for financial assets of changes in monetary policy, business conditions, and political factors. But these price movements cannot be accepted naively as a guide to policy. First, market expectations can simply be wrong; and second, forward prices are artifacts of expectations and arbitrage that, taken together, create a forward price that may be only a distant cousin of the market's "true" expectations.

Consider the case of Germany in 1989–90. In September 1989, before the fall of the Berlin Wall, yields on German 10-year bonds were 6.8 percent; in February, they were 9 percent. The markets demanded 220 basis points in yield within five months, with most of the upward movement in interest rates taking place in one month. Yields rose from 7.5 percent at the start of 1990 to 9 percent by February. During the several years before 1990, Germany's measured inflation varied between zero and 3 percent. Observed inflation during 1990 remained between 2 percent and 3 percent. Yet the markets quickly demanded a rise in real interest rates of two full percentage points.

Despite a great deal of market "move" operating to drive a wedge between forward prices and *ex post* actual prices, some policy realities are driven by market behavior. Central banks that heed and, better still, anticipate market reactions are likely to be rewarded with lower interest rates. Consider the Bank of Japan in the period following the Iraqi invasion of Kuwait. With oil prices nearly doubling and the threat of an oil shock creating imported inflation, the BOJ preemptively raised the discount rate by 75 basis points on August 30, 1990, and guided short rates from 7.5 percent to 8.5

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percent in just six weeks. These moves convinced markets that the BOJ would not tolerate a repetition of the disastrous experience of the mid-1970s when the central bank accommodated the oil shock. With the Japanese central bank moving ahead of market expectations, the long bond yield quickly peaked at 7.8 percent (from 7.0 percent on August 1 to 7.8 percent on September 30) and moved steadily down to 6.25 percent in February 1991. Simultaneously, the currency strengthened from 150 yen per dollar in early August to 125 in mid-October and has subsequently remained in the 125 to 135 range, even as apprehension over a shooting war rose in the months leading to the allied campaign in January 1991.

The BOJ's experience contrasts with the American experience where the Fed began pushing down short-term interest rates in December 1989, with federal funds trading at 8.50 percent and the 30-year bond trading at 7.92 percent. By February 1991, the federal funds rate was about 6.25 percent, the long bond yielded nearly 8 percent, and the dollar fell about 8.5 percent against the yen. Notwithstanding the wisdom of the Fed's decision to seek a soft landing with lower short-term interest rates, the result of a 225 basis point cut in the federal funds rate has been a slight increase in long-term borrowing costs. Fourteen months after a preemptive ease, long-term rates were marginally higher in the United States; whereas six months after Japan's preemptive tightening, long-term interest rates dropped over 160 basis points.

Conclusion

The growth of efficiency of global capital markets over the past decade and a half has had a desirable—and predictable—side effect for central banks. The markets now reward more generously and more rapidly with lower borrowing costs—in both real and nominal terms—those governments whose central banks act convincingly to counter inflation. The painful corollary, of course, is that the markets will punish more severely and rapidly those central banks that say they are committed to fighting inflation but act otherwise. Perhaps this is progress. Perhaps the daily action of global capital markets, as disorderly and messy as those markets sometimes seem, has produced a public good in the form of lower inflation and lower real capital costs for countries whose central banks understand how markets work.

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Monetary Policy, Market Prices, and Supply-Side Forecasting

David Ranson

Introduction

Market prices contain forecasts. In fact, markets cannot avoid forecasting economic variables like growth and inflation in order to arrive at sustainable prices. If the economic forecasts that are embedded in widely available price data can be decoded, much of the paraphernalia of conventional forecasting might be averted. Markets are offering us their own forecasts free of charge. Market prices can serve several roles simultaneously. They are an instrument for forecasting, they are a medium through which government "interventions" can influence the economy, and they are an objective statement about traders' expectations of the future. In this paper, the key market price is the nominal interest rate.

There are good reasons to believe what markets are saying. Markets must be smarter than any single analyst, because they are influenced by all of the information to which traders have access. It should be no surprise to find that they provide the earliest warning of economic events. How to decode them is not so obvious. Which markets are relevent? Which prices should be chosen? What are the mathematics of computing a forecast? Economic theory is of little help in answering such questions, but disciplined empirical investigation may be very fruitful. Many surprises can result.

This paper describes empirical results obtained over the past 10 years or so and suggests a rationale. Relationships are illustrated here as simply as possible, and without resorting to econometric analysis, although they can be supported by more formal statistical tests. Annual data are sufficient to make the most of the main points.

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The evidence outlined in this paper is limited to the U.S. economy since 1950. Exploratory work suggests that the phenomena described may exist in other countries and during other time periods.

My conclusions do not fit into the standard IS–LM framework in any obvious way. Though in some respects counterintuitive, those conclusions are compatible with claims by supply-side economists that economic activity is driven by incentives and disincentives. By "supply-side forecasting," I do not mean forecasting the supply side of the economy. I mean forecasting the economy by means of a model whose interpretation is consistent with what is widely called "supply-side economics."

Supply-siders and their forebears propose a sensitive relationship between output and effective marginal tax rates. Critics sometimes acknowledge a role for incentives in principle. But they are skeptical that, at current tax rates, the linkage is sensitive enough to make a difference. Incentives are harder to discount in a dynamic context. Take consumers, for example. They might insist on spending a set fraction of their income on apparel, liquor, or automobiles. But they are likely to be flexible about where they buy those goods, or when—especially if it affects the taxes they pay or the restrictions they face. Businesses are equally flexible about where, when, and what they sell. Hence, the rivalry of states and localities in attracting business from their neighbors.[1] So even if total output were nearly oblivious to incentives, the allocation of output across geography, among different activities, and across time must surely be sensitive. In this sense, the supply-siders (asserting the power of incentives) and their critics (discounting the effects of tax rates on total economic activity) could both be approximately right.

This paper is concerned with incentives across time. Just as rival fiscal policies help explain variations in growth among geographical domains, so monetary policy helps explain variations in growth among time domains. Fluctuations in real GNP, traditionally identified as the business "cycle," can be viewed as responses to intertemporal incentives. Interest-rate movements associated with the active use of monetary policy appear to be a primary source of these disturbances. The role of monetary policy in stabilizing inflation is a topic outside the scope of this paper. For that purpose, a different set of markets would be relevant for forecasting and policy control—for example, commodity prices. But whether monetary policy is effective in stabilizing inflation or not, this paper will focus on its role in destabilizing the real economy.

[1]For a recent empirical summary, see Genetski and Skorburg (1991).

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Economic Activity and the Federal Funds Rate

My starting point is a fresh look at some perfectly commonplace data. The federal funds rate (like other interest rates) is connected empirically with economic activity in a very curious fashion. Changes in the federal funds rate are associated with both contemporaneous and subsequent changes in real GNP and in different directions.

Table 1 ranks calendar years 1956–89 according to changes in the average interest rate. The growth of real GNP is listed for each year, the following year, and the second year following. The years in which the interest rate rose materially are separated from those in which the interest rate fell, and averages for real GNP growth are calculated. Contemporaneously, real GNP moves in the same direction as the interest rate. After a lag of one or two years, however, real GNP moves in the opposite direction enough to more than offset the initial movement. These differences are statistically significant in more formal tests.

The same seesaw pattern shows up in other calculations. Figure 1, for example, plots real GNP growth rates against changes in the interest rate. The left panel shows contemporaneous, or current, variables, while the right panel reveals a one-year time lag between the interest-rate variable and the GNP variable. The dotted lines show the slopes of the least-squares lines, spanning 95 percent confidence intervals around the data. The contemporaneously positive slope contrasts strongly with the negative slope for the lagged relationship.

Early-Warning Forecasts of the Economy

The lead time between a change in the interest rate and the full associated change in real GNP is estimated to be between one and two years. But some components of the economy (such as the housing industry) usually lead GNP, while others (such as nonresidential construction) lag. Table 2 illustrates the time delay by contrasting the behavior of the federal funds rate in advance of the highest and lowest growth years of the 1952–90 period. The average delay from an interest-rate change to the associated (inverse) real GNP change is suggested by the period for which the contrast is greatest. According to this test, that time is 22 months, give or take a month or two.

Since early 1980 a model based on the similar explanatory power of three-month treasury bill rates has produced regular forecasts of the economy. The model is known as the "Conditional Forecaster Technique" (CFT)[2] because its estimates, termed "market forecasts," are conditional on the market prices of Treasury securities.

[2]The model is described in Ranson (1982, pp. 39–70).

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TABLE 1

FEDERAL FUNDS RATE MOVEMENTS AND THE ECONOMY, 1956–89

Years Ranked in Descending Order of Change in Federal Funds Rate		Year-Over-Year Real GNP Growth		
Calendar Year	Interest Rate Change (basis points)	Same Year (percent)	Following Year (percent)	Second Year Following (percent)
1973	430	5.2	−0.5	−1.3
1979	326	2.5	−0.2	1.9
1981	302	1.9	−2.5	3.6
1969	255	2.4	−0.3	2.8
1978	239	5.3	2.5	−0.2
1980	216	−0.2	1.9	−2.5
1974	178	−0.5	−1.3	4.9
1959	173	5.8	2.2	2.6
1989	165	2.5	1.0	−1.2
1968	144	4.2	2.4	−0.3
1984	114	6.8	3.3	2.7
1966	104	5.8	2.9	4.2
1956	95	2.1	1.7	−0.8
1988	91	4.5	2.5	1.0
1962	73	5.3	4.1	5.3
1965	58	5.8	5.8	2.9
1963	50	4.1	5.3	5.8
1977	49	4.7	5.3	2.5
1957	38	1.7	−0.8	5.8
1964	32	5.3	5.8	5.8
1960	−8	2.2	2.6	5.3
1987	−15	3.4	4.5	2.5
1972	−23	5.0	5.2	−0.5
1976	−78	4.9	4.7	5.3
1967	−89	2.9	4.2	2.4
1970	−103	−0.3	2.8	5.0
1961	−127	2.6	5.3	4.1
1986	−130	2.7	3.4	4.5
1958	−154	−0.8	5.8	2.2
1985	−212	3.3	2.7	3.4
1971	−252	2.8	5.0	5.2
1983	−317	3.6	6.8	3.3

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TABLE 1 (cont.)

FEDERAL FUNDS RATE MOVEMENTS AND THE ECONOMY, 1956–89

Years Ranked in Descending Order of Change in Federal Funds Rate		Year-Over-Year Real GNP Growth		
Calendar Year	Interest Rate Change (basis points)	Same Year (percent)	Following Year (percent)	Second Year Following (percent)
1982	−412	−2.5	3.6	6.8
1975	−468	−1.3	4.9	4.7
Averages for 12 years that interest rate rose more than 100 basis points	220	3.5	1.0	1.6
9 years that interest rate fell more than 100 basis points	−242	1.1	4.5	4.4

It relies on futures prices to provide the market's own expectations of future spot prices.

These interest-rate linkages capture a large part of the post–World War II fluctuations in real GNP that we call the business cycle. Figure 2 back-tests the power of the CFT to explain changes in real GNP growth over the postwar period. More than two-thirds of the year-to-year variation is picked up by the model. The CFT has also been able to forecast hundreds of GNP components and other economic variables that change as the business cycle runs its course. These forecasts, obtained from interest rate data alone, have succeeded in anticipating most of the economy's turning points during the 1980s.

The upper panel of Figure 3 compares the growth rate of real GNP with the market forecast published from the CFT in the middle of the previous year. The model erred in 1986–87[3] but tracked the economy surprisingly well otherwise, taking into account the large

[3]These errors are also visible in Figure 2, which reveals an suggestive symmetry between underestimation of growth in 1985 and overestimation in 1986–87.

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FIGURE 1

INTEREST RATES AND THE ECONOMY: A SEESAW RELATIONSHIP

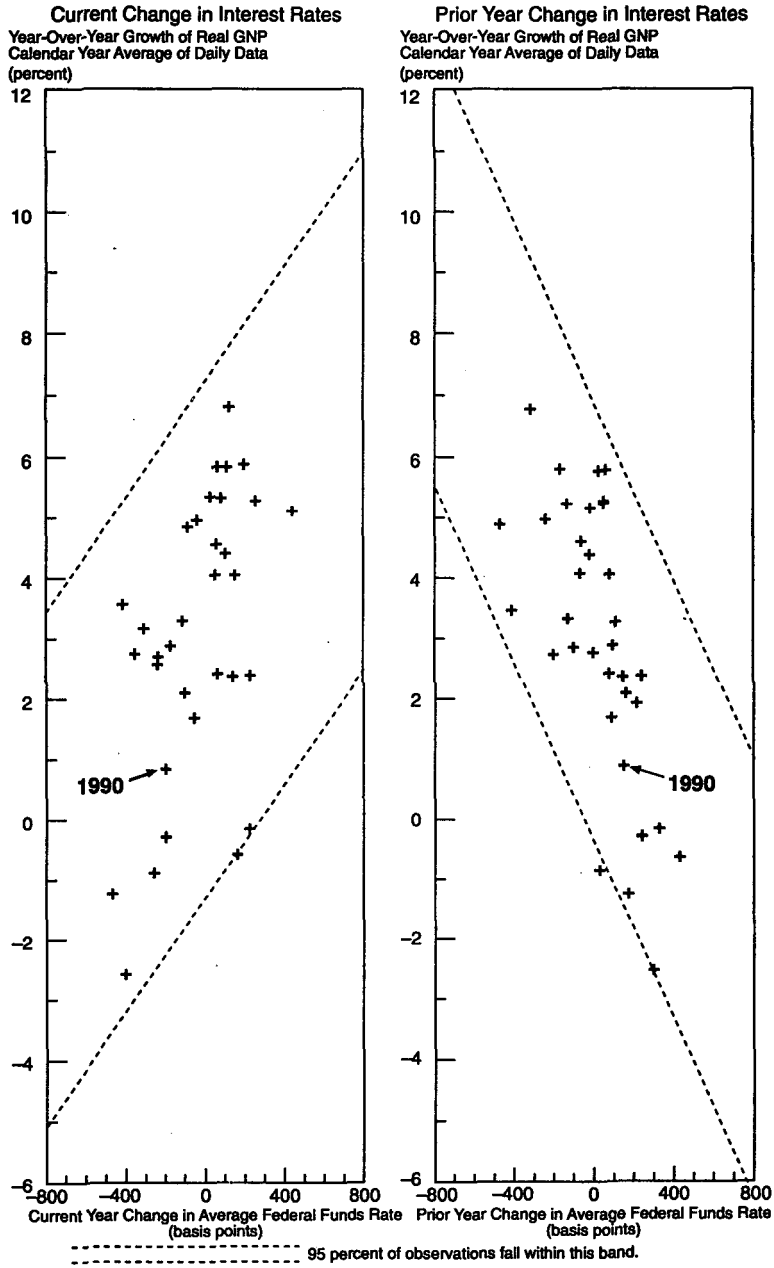

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TABLE 2

HOW EARLY THE FEDERAL FUNDS RATE MOVES IN ADVANCE OF GNP, 1952–90

		Fastest 13 Years of Real GNP Growth[a]	Slowest 13 Years of Real GNP Growth[b]	
Average Year-Over-Year Change in Real GNP (percent):		5.4	0.3	
Lead Time (months)	End of Month	Average Annual Change in the Federal Funds Rate[c]		Difference (percentage pts.)
0	June	1.0	−1.2	+2.2
2	April	0.9	−1.1	+2.1
4	February	0.9	−0.9	+1.8
6	prior December	0.7	−0.7	+1.5
8	prior October	0.5	0.1	+0.5
10	prior August	0.4	0.5	−0.1
12	prior June	0.1	0.8	−0.7
14	prior April	−0.2	1.1	−1.3
16	prior February	−0.5	1.3	−1.8
18	December 2 years prior	−0.7	1.4	−2.2
20	October 2 years prior	−0.8	1.5	−2.3
22	August 2 years prior	−1.0	1.3	−2.4
24	June 2 years prior	−1.0	1.1	−2.1
26	April 2 years prior	−.9	0.9	−1.8
28	February 2 years prior	−.7	0.8	−1.5

[a]The fastest growth occurred in 1984, 1959, 1965, 1966, 1955, 1964, 1962, 1978, 1973, 1972, 1976, 1977, and 1978.
[b]The slowest growth occurred in 1969, 1960, 1956, 1981, 1957, 1990, 1980, 1970, 1974, 1958, 1975, 1954, and 1982.
[c]The Fed funds rate is *both* a leading indicator and a coincident indicator of real GNP. With a lead time of 1.5 to 2 years, changes in the Fed funds rate anticipate movements in GNP in the opposite direction. Simultaneously, however, Fed funds rates track movements in GNP in the same direction.

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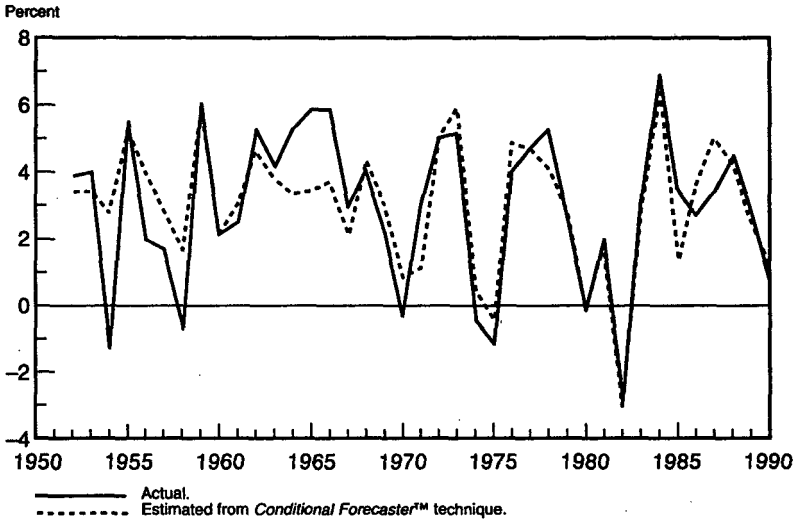
FIGURE 2

HOW INTEREST RATES TRACK REAL GNP



number of explanatory variables it omits. The lower panel repeats this exercise using the consensus forecasts published at the same time by *Blue Chip Economic Indicators*.[4]

The graphs reveal a defect of the CFT in its original form. Over the 1980s the market forecasts were consistently optimistic by about .4 percent. The model from which these forecasts were derived allowed for no change in the relationship between the variables over the whole 1950–90 period. However, it appears that the secular growth rate of the economy has declined somewhat since the 1950s. Statistical tests confirm a downward shift, probably around the end of the 1960s, which is just significant at the 95 percent level of confidence. The CFT has now been modified to reflect this finding. In other respects, however, the relationship between interest rate movements and real GNP growth appears to be statistically stable from the 1950s through the 1980s.

The model is most effectively tested under unusual conditions. For the years in which economic growth departed furthest from the norm of about 3 percent growth, forecasts from the CFT have notably outperformed conventional forecasting models monitored by the Blue Chip survey. Table 3 illustrates the range of opinion at the midpoint of each prior year. The CFT tended to diverge from the

[4]Published monthly by Eggert Economic Enterprises, Sedona, Arizona.

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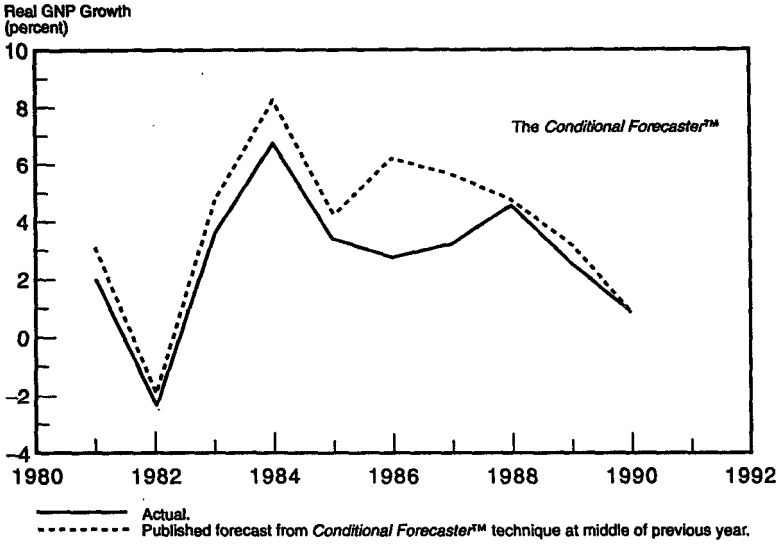
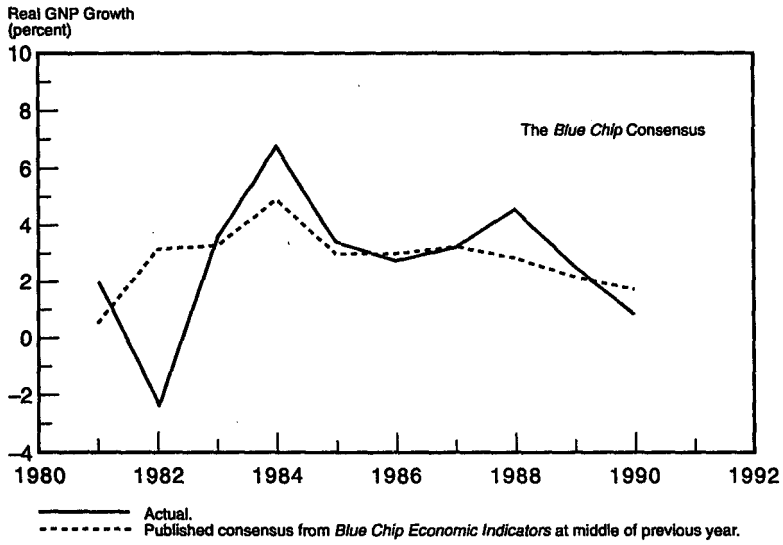
FIGURE 3

FORECAST TRACK RECORD IN REAL TIME





norm much more than popular forecasting models. In these years, both the actual outcome and the CFT's market forecast were outside the range of opinion covered by Blue Chip. Table 4 suggests that "market forecasts" from the CFT were directionally correct as early

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TABLE 3

FOUR TURNING POINTS OF THE 1980s[a]: FORECASTERS' ESTIMATES AT THE MIDDLE OF THE PREVIOUS YEAR

	1982 (percent)	1984 (percent)	1988 (percent)	1990 (percent)
Actual real GNP growth	−2.5	6.8	4.5	1.0
Forecasts				
Blue Chip average, high 10 forecasts	4.2	6.1	4.0	2.6
Blue Chip consensus	3.2	4.9	2.8	1.5
Blue Chip average, low 10 forecasts	2.1	3.9	1.3	1.0
Wharton Econometric Forecasting Associates	3.0	5.4	3.0	2.4
Chase Econometrics	3.4	4.6		
University of Michigan	NA[b]	6.2	2.4	1.6
Data Resources	3.1	4.6	2.5	1.2
Wainwright Economics market forecast[c]	−2.1	8.4	4.7	0.9

[a]Calendar years whose growth diverged most from the 3 percent norm.
[b]NA = Not available.
[c]Wainwright Economics' market forecast tended to diverge from the norm of 3 percent growth much more than popular forecasting models. In these years, both the actual outcome and the market forecast were virtually off the scale of the Blue Chip survey of 40 or 50 forecasters.

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TABLE 4

EARLY WARNING FORECASTS FOR THE FOUR MAJOR TURNING POINTS OF THE 1980s[a]

	1982 (percent)	1984 (percent)	1988 (percent)	1990 (percent)
Actual real GNP growth	−2.5	6.8	4.5	1.0
Blue Chip consensus published on				
January 10 of target year	0.3	5.3	2.2	1.7
October 10 of prior year	2.2	5.1	2.8	1.8
July 10 of prior year	3.2	4.9	2.8	1.5
March 10 of prior year	3.7	4.6	3.3	1.7
October 10, 2 years prior	NA[b]	4.0	3.3	1.7
Wainwright Economics market forecast published during				
October of prior year	−2.0	8.2	5.1	0.8
July of prior year	−2.1	8.4	4.7	0.9
April of prior year	1.4	8.1	4.4	0.2
January of prior year	1.2	9.3	5.0	1.4
October 2 years prior	1.7	6.2	5.3	2.1

[a]Calendar years whose growth diverged most from the 3 percent norm.
[b]NA = Not available.
NOTE: Market forecasts that were based on interest-rate data gave a strong signal, and were directionally correct as early as 2 years in advance. The Blue Chip consensus underpredicted the magnitude of the turning points until very late.

as two years in advance. In contrast, the Blue Chip consensus underpredicted the magnitude of the turning points until very late.

Money Supply, Interest Rates, and the Yield Curve

Though it relies on monetary data, the CFT is not a monetarist model, and the quantity of money plays no role in it. This omission need not imply disbelief in the role of the monetary aggregates in the economy. Reliance on interest-rate data simply reflects the notion that prices provide better and earlier information than quantities. Indeed, interest-rate movements precede changes in the money supply by a substantial lead time and not vice versa.

Figure 4 illustrates this point by repeating the exercise in the right panel of Figure 1 while using the growth rate of real M1 instead of real GNP (left panel). An inverse relationship is unmistakable. The right panel of Figure 4 reverses the timing, showing the link between money supply changes and changes in the interest rate one year later: a less impressive relationship. Formal statistical tests confirm that the first relationship is statistically significant, and the second is not. This finding does not contradict the view that the monetary aggregates are legitimate leading indicators of the business cycle. But it does show that interest-rate movements provide similar information sooner. Moreover, when changes in real M1 are added to an equation that relies on interest-rate movements to explain real growth, they are not statistically significant. It seems that their value as leading indicators is co-opted by interest-rate movements that have already occurred.

Although operationally different from monetarist models, the model outlined here is sympathetic to monetarism. It emphasizes rational behavior in free markets and endorses the monetarist warning against fine-tuning. On the other hand, it points to price rules rather than quantity rules in the conduct of monetary policy.

This model is not a vehicle for the widely known forecasting power of the yield curve. The yield spread between treasury bonds and the federal funds rate anticipates the growth of the economy about two quarters ahead[5], whereas the delay between changes in the funds rate and economic growth averages over a year.

Over the past 10 years, many other economic variables have been tested as candidates for inclusion in the model—for example, changes in equity prices, in oil prices, and in the stock of debt. Very few, if any, of these have boosted the explanatory power of interest-rate movements alone.

[5]For recent work, see Laurent (1990).

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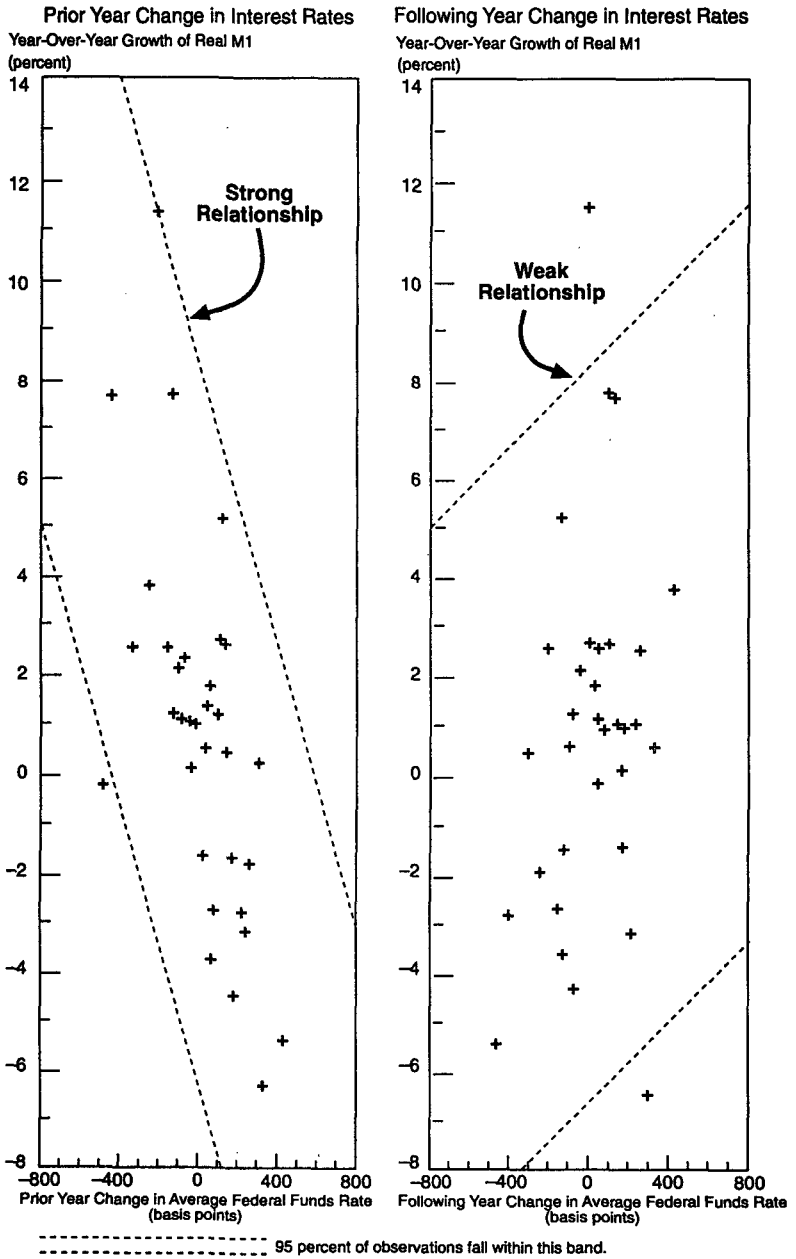
FIGURE 4

INTEREST RATES AND THE REAL MONEY SUPPLY: WHICH LEADS AND WHICH LAGS?


Prior Year Change in Interest Rates
Year-Over-Year Growth of Real M1
(percent)
Strong Relationship
Prior Year Change in Average Federal Funds Rate
(basis points)
Following Year Change in Interest Rates
Year-Over-Year Growth of Real M1
(percent)
Weak Relationship
Following Year Change in Average Federal Funds Rate
(basis points)
95 percent of observations fall within this band.

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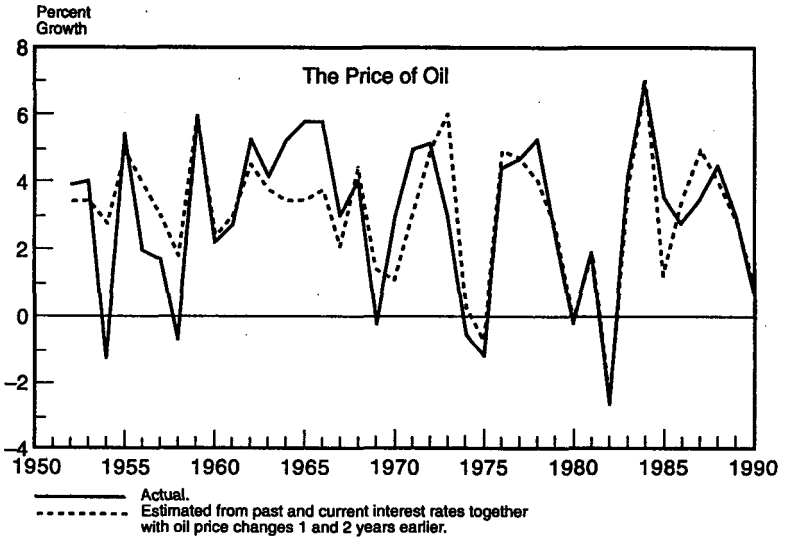
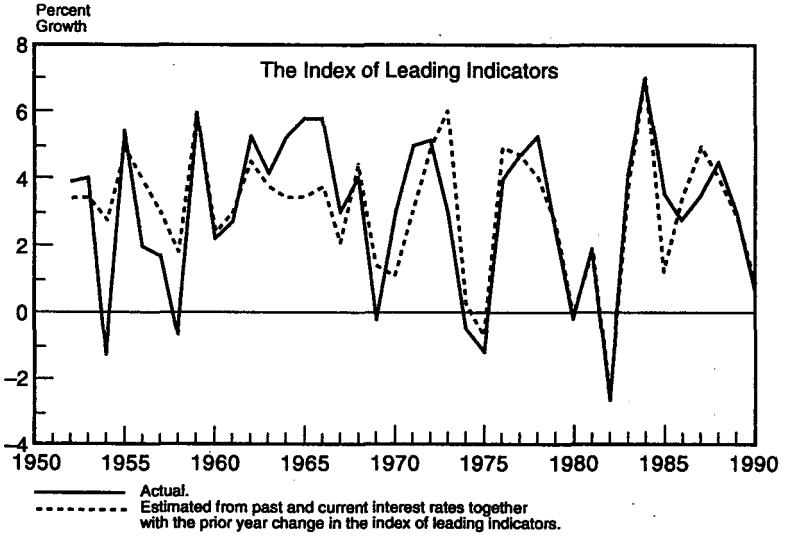
The first panel of Figure 5 shows the effect of incorporating annual changes in the price of oil within the CFT equation expressed in Table 2. The second panel uses instead the lagged change in the index of leading indicators. In neither case is a major

FIGURE 5

TWO ADDITIONAL VARIABLES OF MARGINAL SIGNIFICANCE





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improvement in the fit perceptible to the unaided eye. More formal tests show each of these plausible predictors of real GNP to be statistically insignificant in the presence of the interest-rate variables.

Leading indicators such as the money supply growth, the yield curve slope, and inventory changes bear a one-way relationship with subsequent movements in the economy. But interest rates are different. Each change in the interest rate is associated with two changes in the economy: one roughly immediate and the other long delayed. Thus, interest-rate movements are not a conventional leading indicator. The interpretation suggested here is that interest-rate changes cause GNP to migrate from one time period to another. A fall in the interest rate causes GNP to be postponed—to migrate from the present into the future. Symmetrically, a rise in the interest rate causes part of future GNP to be brought forward into the present.

Table 5 provides evidence of such migration during the 1970–90 period. It compares what happened to real GNP two years after it grew rapidly with what happened two years after it declined or grew slowly. The inverse correlation is clear and is statistically significant as well. An inverse relationship is also detectable over the 1950–70 period, but annual data do not provide a statistically significant correlation over this longer period.

Figure 6 further illustrates the power of this mechanism to explain cyclical fluctuations in the economy during the 1970s and 1980s. The chart compares the actual history of real GNP growth with the following simple least-squares formula that fits the data quite well:

$$\begin{pmatrix}\text{real}\\ \text{GNP}\\ \text{growth}\end{pmatrix} = 5.3\% \text{ minus } 0.5 \begin{pmatrix}\text{real growth}\\ \text{2 years}\\ \text{ago}\end{pmatrix} \text{ minus } 0.5 \begin{pmatrix}\text{real growth}\\ \text{2 years}\\ \text{ahead}\end{pmatrix}$$

Both explanatory variables are statistically significant at well past the 95 percent level of confidence. The formula implies that real growth would have been substantially more stable if migration among time periods were eliminated.

The idea of GNP migration over time casts an unfamiliar and nonintuitive light on current debate over monetary policy. An interest-rate reduction may be of no help to the economy—either in the long run or the short. Far from hastening recovery from recession, a further sharp reduction in nominal interest rates would postpone it by further stimulating the emigration of current GNP into the future. If the migration view is correct, the route to the earliest possible recovery would be restoration of interest-rate stability.

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TABLE 5

REAL GNP GROWTH IN THE PRESENT AND THE FUTURE: AN INVERSE CORRELATION

Year	Growth in Annual Real GNP	
	Same Year (percent)	2 Years Earlier (percent)
1984	6.8	−2.5
1978	5.3	4.9
1973	5.2	2.8
1972	5.0	−0.3
1976	4.9	−0.5
1977	4.7	−1.3
1988	4.5	2.7
1983	3.6	1.9
1987	3.4	3.3
1985	3.3	3.6
1971	2.8	2.4
1986	2.7	6.8
1989	2.5	3.4
1979	2.5	4.7
1981	1.9	2.5
1990	0.9	4.5
1980	−0.2	5.3
1970	−0.3	4.2
1974	−0.5	5.0
1975	−1.3	5.2
1982	−2.5	−0.2
Medians for		
7 high growth years	5.0	−0.3
7 average growth years	2.8	3.4
7 low growth years	−0.3	4.5

Table 6 provides further evidence on this point. Sudden movements in interest rates coincide closely in timing with sudden movements in real GNP, and in the same direction. On 13 occasions since data collection began in 1955, the federal funds rate has dropped more than 100 basis points from one quarter to another. During those same quarters, real GNP fell at a median annual rate of more than 3 percent. As always, statistical results cannot show causation, but can only suggest it. It is impossible to be sure to what extent this correlation reflects the response of the Fed to the economy, and to

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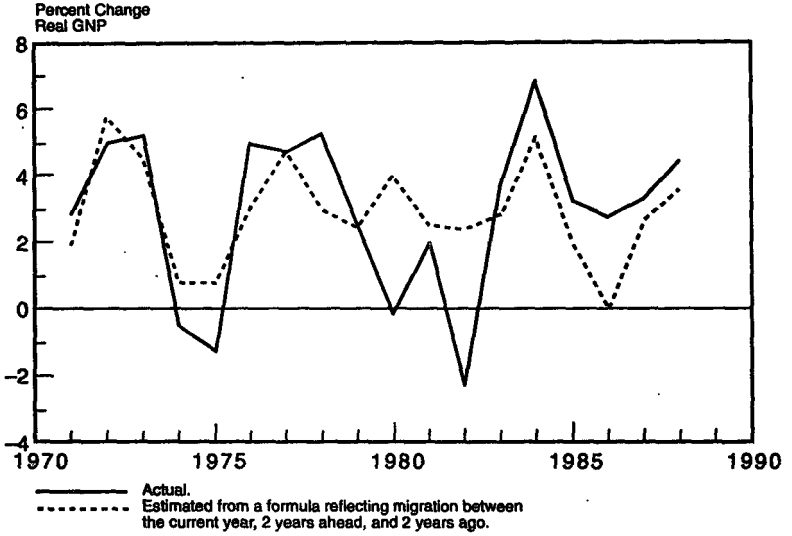
FIGURE 6

EVIDENCE FOR MIGRATION: ITS CONTRIBUTION TO THE CYCLICAL BEHAVIOR OF REAL GNP
1971–88



what extent it reflects the response of the economy to the Fed. Both factors may be at work.

How such incentive effects on the real economy might be accomplished by changes in nominal interest rates is at first puzzling. Understanding the puzzle demands a different way of interpreting an interest-rate change. But if the interest rate is viewed as a statement by the market about expectations of the future, these statistical results begin to make sense, as explained next.

A General View of Migrating GNP

The view of the economy emerging from these observations is the opposite of the old view that the private sector is inherently unstable. Instead, we should entertain the polar opposite: (a) that, abstracting from secular growth, aggregate real GNP is stable in undisturbed free markets, and (b) that monetary policy introduces much of the instability that is observed.

Economic activity is always free to migrate. In one way or another, GNP votes with its feet. Like capital and population, it migrates from unfavorable to favorable climates. As a result, we observe more GNP than expected in one circumstance and less than expected in

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TABLE 6

THE SHARPEST DECLINES IN FEDERAL FUNDS RATE: WHAT HAPPENED TO THE ECONOMY?

Quarter	Quarter-Over-Quarter Change in Fed Funds Rate (basis points)	Quarter-Over-Quarter Change in Real GNP (SAAR)[a] (percent)
1981 IV	−399	−5.5
1982 III	−351	−3.2
1975 I	−304	−7.6
1980 III	−285	0.3
1974 IV	−274	−3.5
1980 II	−236	−9.1
1984 IV	−212	1.7
1982 IV	−172	0.6
1971 I	−171	11.2[b]
1958 I	−137	−7.9
1972 I	−121	9.1
1970 III	−118	5.0[b]
1970 IV	−114	−3.6[b]
Medians	−212	−3.2

[a]SAAR = Seasonally adjusted annual rate.
[b]GNP may have been distorted during these quarters by a General Motors strike that occurred during the fourth quarter 1970.
NOTE: Included are all quarters in which the quarter-average federal funds rate dropped by more than 100 basis points.

another. Thanks to escape through migration, the effects of government policies may often be undesired and destabilizing.

Economic stability can be disturbed in at least four distinct ways. As incentives change, economic activity migrates between (1) the legal economy and the underground economy; (2) one country, region, or locality and another; (3) one industry, industrial segment, or class of institution and another; and (4) one time period and another.

First, consider a closed economy whose government imposes such severe taxes or restrictions on the use of private property that legitimate economic activity is unattractive to the population. In that case, much of GNP migrates underground. Those activities that continue in the legitimate sphere will tend to be the ones on which the government is least able or inclined to enforce its hostile policies.

Second, GNP can also migrate from one place to another among countries or within the same nation state, reflecting the tax

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or regulatory postures of different local jurisdictions. Hence, the celebrated migration of economic activity from Massachusetts to New Hampshire or elsewhere when tax burdens rise in Massachusetts.

Third, GNP can migrate among industries, among sectors within industries, or among classes of institution, depending on the specific policies government adopts toward them. The banking industry serves as a topical example. Traditional commercial banks are succumbing to zealous efforts by the bank examiners to write down their doubtful assets; at the same time, those banks face tighter capital adequacy ratios. One predictable consequence is a loss of market share in the credit industry to other institutions that can readily create credit.[6] In all three of these instances, the operative principle is *substitution*. Each of these various leakages is a form of arbitrage.

Fourth, and less familiarly, GNP can migrate from one time domain to another. Here the motivating force is not exactly arbitrage but a parallel urge to exploit expectations about the future. The ability to recognize this process is hampered by a paradox. For example, the announcement of an increase in tax rates beginning with the next tax year would stimulate economic activity in the present.[7] "The farmer makes hay while the sun shines." Symmetrically, news of a tax cut yet to come enhances the future economy at this year's expense. So good news about the future climate, ironically, can be a temporary depressant to GNP.

Le Chatelier's Principle

Thanks to greater awareness of GNP migration, the power of government to determine the level of economic activity is not as credible as it was a generation or more ago. Economic agents find it second nature to look for escape routes, leakages, or opportunities to substitute whenever they are crimped by government policy.

The basic principle was formalized in the physical sciences more than a century ago by a French chemist, Henry-Louis Le Chatelier (1850–1936). Le Chatelier's principle provides that "When a constraint is applied to a dynamic system in equilibrium, a change takes place within the system, opposing the constraint and tending to

[6]C. J. Lawrence, Morgan Grenfell Inc. has documented a consistent market share loss in the past decades (see Malabre and Clark 1990).

[7]Such a situation occurred in spring 1979 in Britain when Margaret Thatcher's new government announced a July 1 hike in the value-added tax from 8 to 15 percent. This tax window was exploited by an unprecedented surge in spending. In 1991 the experiment was repeated, as the Major government gave a few weeks' warning of a further increase in VAT to 17.5 percent.

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restore equilibrium" (Pitt 1977, p. 217). The usual physical illustration is a compressed balloon. When pressure is applied, the shape of the balloon may be distorted in many ways without much reduction of the volume of air inside it.

Especially in the field of money and credit, belief in "government as provider" dies hard. However, it may be a myth that real GNP is brought into existence by "loose" policy and snuffed out by "tight" policy. An ability to create and destroy GNP, like air inside a balloon, is at the heart of the Fed's assumed powers. In recessionary times, the Fed is pressed to get the economy moving again. Over the long haul the Fed is expected to keep the path of GNP stable. Even those keenly alert to the destabilizing effects of Fed policy usually blame its failures on policy mistakes rather than on an automatic response of the economy to defeat the policy. According to this view, monetary policy may be *inherently* destabilizing. That view could help to explain why, in spite of decades of research and practical experience, Fed chairmen still preside over recessions. Perhaps "loose" (low interest rate) and "tight" (high interest rate) policies do not work to create or destroy GNP after all. They may succeed merely in rescheduling it.

The idea of migration casts an unfamiliar and counterintuitive light on the current debate over monetary policy. An interest-rate reduction is not helpful to the economy—either in the long run or even, paradoxically, the short. Far from hastening recovery, a further rapid reduction in nominal interest rates would postpone it by further stimulating the emigration of GNP into the future. If the migration view is correct, the route to the earliest possible recovery would be immediate interest-rate *stability*.

Real Effects from Nominal Interest Rates

Empirically, a change in the nominal interest rate influences real GNP in the same fashion as the announcement of a future tax change. What possible connection exists between the price of credit and expectations of future tax rates? The answer is direct but, perhaps, unfamiliar. At least since Irving Fisher ([1930] 1986, p. 41ff), economists have recognized the nominal interest rate as the market's statement of its expectations regarding the future purchasing power of the dollar. And changes in the purchasing power of the dollar—that is, inflation—imply changes in tax rates.

Supply-side economists have long emphasized this collision between inflation and progressive tax schedules.[8] The fact that

[8]Complaints about the distortive effects of a tax structure designed originally for a no-inflation economy are, of course, much older than supply-side economics. Some of the earliest attention in the literature occurs in Britain, where both inflation and tax

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inflation drives the economy into steadily higher tax brackets received some attention from the Reagan administration, but the federal tax authorities have done little as yet to eliminate the problem. Even after the indexation of personal tax brackets to the consumer price index, several features of the tax code remain highly sensitive to inflation. Thanks in part to the capital gains tax and the mandatory use of historic cost accounting, inflation continues to drive businesses and individuals into higher effective tax brackets (Ranson 1982, p. 48ff).

FIGURE 7

WHAT MAKES THE MODEL WORK?

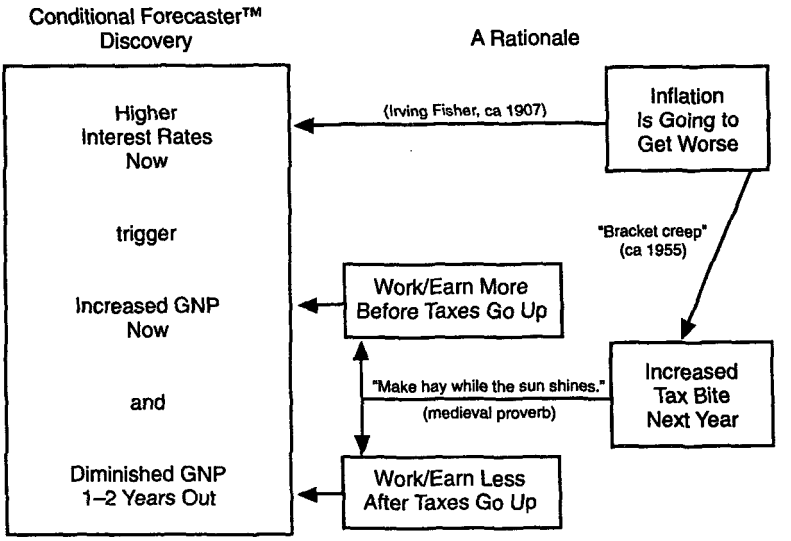

Figure 7 diagrams the theoretical linkages in plain language. Three widely recognized truths are connected in an unfamiliar way to support the conclusion documented in the U.S. postwar evidence outlined above. The three linkages are the following:

1. The market nominal rate of interest reflects the expected rate of decline in the purchasing power of the dollar.
2. The lower the purchasing power of the dollar, the more incomes are overstated for tax purposes—thanks to improperly designed tax formulas.

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progressivity were steeper after World War II than in the United States (see Myddelton 1969, pp. 102, 107, 112).

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3. A change in expected future tax rates alters the balance of incentives to produce output now or in the future.

Hence, a change in the market rate of interest transfers output from one time period into another.

Conclusion

The short-term interest rate is a fascinating, powerful, and underused predictor of the U.S. economy. Because it is a market price, its lead time is longer than that of other leading indicators. And its relationship to economic activity is unlike theirs. Fluctuations in nominal interest rates trigger a "bouncing" process in the economy, trading off one year's real GNP against GNP in future years. The tradeoff reflects the opportunity to postpone or expedite production. Incentives to bounce are driven by expectations of the rate at which inflation will drive up effective marginal tax brackets.

Since 1980, real-life forecasts based on movements in treasury bill rates have anticipated economic turning points better and earlier than many other approaches. For example, in winter 1989, this model warned of the 1990 recession. However, its logic suggests that strong efforts by the Federal Reserve to push interest rates down will delay the upturn.

This paper has suggested that monetary policy has incentive effects on the economy that have not been widely recognized. Changes in market prices, such as the federal funds rate, produce seesaw movements in the economy:

- Interest-rate movements provide exceptionally early warning of changes in real GNP.
- The relationship is very tight.
- The time lag from the Fed funds rate to real GNP is at least a year.
- It is as if GNP migrates from one time period to another when interest rates change.
- A sharp decline in the Fed funds rate is associated with *more* GNP a year or more from now, but symmetrically *less* in the immediate future.

Intertemporal migration of GNP is an instance of LeChatelier's principle. In the field of political economy, this principle implies that, while the government has enormous power to change the shape of the economy, government can do little to alter the economy's size.

These thoughts shed new and unfamiliar light on old problems. How sharply should the Federal Reserve force interest rates down

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so as to hasten recovery from recession? The answer would be "Not at all." In the short run, a reduction in interest rates postpones recovery. At the same time, this paper supports an old, familiar conclusion. Monetary fine-tuning is dangerous because it countermands the natural role of free markets in keeping the economy stable.

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Supply-Side Forecasting: Theory and Practice

Robert J. Gordon

Introduction

David Ransom has written two papers. The first paper endorses the short-term nominal interest rate as a "fascinating, powerful, and underused" predictor of output growth in the U.S. economy. The second paper proposes a theoretical supply-side explanation of the forecasting results of the first part, arguing that changes in the interest rate induce intertemporal migration of economic activity. Since the two papers are largely independent, I will discuss them separately.

If the first paper is correct, Ranson's predictive results have profound implications for the forecasting community. There is no need for large models, nor for the hundreds of professional economists in private corporations, in government departments, or at the Federal Reserve. For Ranson's model not only performs better than such comparison groups as the Blue Chip forecasters, but also is so simple that it can be employed without professional help by any chief executive owning a personal computer. His paper might be relabeled to carry the subtitle: "Economic Forecasters' Unemployment Act."

Before turning to the details, let me summarize my evaluation of the two papers. Regarding the first, Ranson's new forecasting tool for the U.S. economy, as presented, is plagued by simultaneity between the contemporaneous changes in the nominal interest rate and real GNP. Variants that either omit the current interest rate term or replace it with an interest rate forecast fit the data much worse. All versions fail miserably in forecasting the weak economy in 1991, a failure that becomes more obvious when quarterly versions of the equations are estimated. This failure is shared by other forecasting

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methods that rely heavily on interest-rate changes or interest-rate spreads, as in the poor performance of the NBER experimental leading indicator developed by James Stock and Mark Watson. These two related forecasting tools, Ranson and Stock-Watson, have failed for the same reason. Summarizing my critique of the second paper, the theoretical interpretation, the proposed mechanism is at best a second-order effect and probably does not exist at all.

Forecasting Real GNP with the Short-term Interest Rate

Much of Ranson's paper advertises the advantages of a forecasting equation relating the change in real GNP to the current and lagged change in the federal funds rate. However this equation is never actually written down in this paper, and I found it only by searching through earlier papers using the same technique. This earlier work, dated 1980, used the treasury bill rate rather than the federal funds rate, so all the results I report here use the treasury bill rate. The basic Ranson (1980) result, which he estimated for 1952–79, is displayed on the first line of Table 1, reestimated with current data through 1991. Indeed, in annual data the current change in the short-term interest rate has a high positive correlation with the current percentage change in real GNP, and the first annual lag has a high negative correlation. However, any econometrician would be disturbed by the inclusion of the current interest rate change in the equation, because the current change must be positively correlated with the error term in the equation, introducing simultaneity. A high realized value of real GNP, for whatever reason, raises the demand for money relative to the supply and raises the interest rate, as long as the Fed is pursuing any policy other than rigid pegging of the short-term interest rate.

These regressions are extremely simple, containing only a constant, a current interest-rate change, and a lagged interest-rate change.[1] One of his three coefficients is illegitimate. It is also useless. Forecasting cannot take advantage of the contemporaneous coefficient, since one cannot predict the change in real GNP for the year 1991 until all the data on the 1991 short-term interest rate are in. It is hardly a triumph to forecast real GNP for 1991 on January 1, 1992! The next line in Table 1 shows that when the contemporaneous interest rate term is dropped, the $\overline{R}^2$ of the equation falls by half, although the coefficient on the lagged interest rate change is quite stable.

[1]My versions also include a constant shift term for the period after 1972, following a suggestion in Ranson's text. This term is never significant at the 5 percent level in any version reported in Tables 1 and 3.

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TABLE 1

ANNUAL REGRESSIONS OF PERCENTAGE CHANGE IN REAL GNP ON CURRENT AND LAGGED CHANGE IN NOMINAL TREASURY BILL RATE

	Coefficient on Δi					Forecast Error, Sample Ends 1989		
Time Period	Actual at t	Predicted at t	Actual at t-1	$\bar{R}^2$	S.E.E.	1990	1991	Mean Error
1952–91	0.89**	—	−1.09**	0.62	1.44	0.43	−2.34	−0.95
	—	—	−0.94**	0.32	1.92	−0.45	−4.16	−2.31
1982–91	0.95**	—	−0.74**	0.63	1.59	−0.60	−2.61	−1.60
	—	−0.09	−0.77*	0.20	2.33	−0.60	−3.83	−2.22
	—	—	−0.86*	0.29	2.19	−0.21	−3.71	−1.96

NOTE: All equations also contain a constant term, and those for the 1952–91 sample period include a constant shift term for 1973–91. Statistical significance at the 5 percent level is indicated by * and at 1 percent by **.

The use of the contemporaneous interest-rate term can be salvaged, however, with the use of interest-rate forecasts. Ranson has provided me with his unpublished forecasts of the annual interest rate for 1981–91 made at various dates in advance, and I am using those made in the middle of the previous year—a method that is consistent with Ranson's Figure 3. The bottom section of Table 1 contrasts three different equations covering the maximum sample period for which the predicted interest rate changes are available, 1982–91. Here, the first equation includes the illegitimate current interest-rate term and the second replaces it with the predicted interest rate. The $\overline{R}^2$ with the predicted interest rate drops by two-thirds and is even lower than the third version that uses only the lagged interest-rate term; the coefficient on the current predicted interest-rate change is completely insignificant.

In short, many of the correlations displayed in the paper reflect the positive feedback from GNP to the current interest rate and fall apart when the current interest rate is either omitted or replaced with the predicted interest rate. Also notable is the forecasting failure of every version of the equation in Table 1 for 1991, since every version forecasts robust growth of real GNP in 1991 instead of the very slow growth that actually occurred.

The forecasting errors from three versions of the equations for 1982–91 are shown in Table 2. The first two versions correspond to the first two lines of Table 1 but truncate the sample period at 1981, so that information from the forecasting period is not used in the estimated equation. The final version uses the predicted interest rate and is estimated for 1982–89. The standard deviation of the forecast errors for 1982–89 is similar to that of the Blue Chip forecasters and is almost double that of the Ranson forecasts displayed in the top frame of his Figure 3. Unfortunately, there is no way that the relatively accurate Figure 3 forecasts can be replicated with any equation, using either actual or predicted data for the current rate change.[2]

[2]Reading forecasting errors off Ranson's Figure 3, the 1982–90 standard deviation of the forecast error for the Conditional Forecaster in the top frame is 1.0, the Blue Chip in the bottom frame 2.0, and the three versions of our equations in Table 2 are 1.7, 1.9, and 1.8, respectively. But there is no way to replicate the low forecasting errors of the top frame of Figure 3 with any permutation of these equations. For instance, I tried to take the estimated coefficients for 1952–81 and forecast for 1982–91 by applying the estimated coefficient on the current rate change to data on the predicted (rather than actual) current rate change. This led to huge forecasting errors and the prediction that real GNP growth would be negative in both 1983 and 1984 (because the predicted rate series rises sharply from 1981 to 1982 and peaks in 1983, instead of peaking in 1981 like the actual rate series).

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TABLE 2

FORECASTING RECORD, 1982–91, SELECTED ANNUAL EQUATIONS

		Forecast Errors (Actual minus Predicted in %)		
		Sample 1952–81		Sample 1982–89
	Change in Actual Real GNP (%)	Uses Actual Current, Lagged Δi	Uses Only Lagged Δi	Uses Predicted Current, Actual Lagged Δi
1982	−2.6	1.7	−3.1	−2.8
1983	3.5	−1.2	−3.8	−1.1
1984	6.6	0.4	0.7	2.0
1985	3.3	4.2	1.0	2.0
1986	2.7	−0.9	−3.1	−2.0
1987	3.4	−0.9	−1.8	−0.6
1988	4.4	0.9	0.8	1.9
1989	2.5	−0.2	0.1	0.6
1990	1.0	1.1	−0.7	−0.6
1991	−0.8	−1.9	−4.9	−3.8
Std.dev., 1982–89		1.8	2.0	1.9
Std.dev., 1990–91		2.1	2.9	2.3

Any commentator would be mystified at a paper that limits itself to forecasting annual growth rates. There cannot be much of a market for that in the forecasting fraternity, when real GNP is published quarterly, and much hangs on the quarterly announcements of real GNP and its subsequent revisions. Table 3 shows the results of regressions with quarterly data for the full period runing from 1955 to 1990, and three subperiods. A search revealed that up to 9 lags on the interest rate change are necessary, but (omitting the current value to avoid simultaneity) the first is insignificant and is omitted here.

The low $\bar{R}^2$s of these equations are to be expected, since the one-quarter change in real GNP is a very noisy series. The sum of the lagged interest-rate coefficients is highly significant and uniformly larger than the coefficient on the lagged term in the annual equations of Table 1. Interestingly, the subperiod results show that the interest-rate effect declines in size in the later subperiods. These lower coefficients are consistent with the view that financial deregulation has raised the volatility of interest rates relative to the volatility of real GNP (which was unusually low after 1982). The bottom two rows of Table 3 show that the real interest rate performs much worse than the nominal rate, even if allowance is made for the effect on real GNP of supply shocks (which replace the normal negative correlation between the real interest rate and GNP with a positive correlation, as supply shocks drive up inflation and drive down the real interest rate and real GNP).

The abysmal 1990–91 forecasting record of all versions of these equations is evident. Because interest rates fell substantially from 1989 to 1991, the equations predict that real GNP growth would be rapid in 1991. Most versions predict growth in late 1991 and early 1992 at a 5 percent annual rate, instead of the stagnation that actually occurred.

What are the implications of the poor forecasting performance for 1990–91? This failure of real GNP to respond to a steady fall in short-term interest rates since early 1989 tells us that the current episode is not a garden-variety recession caused by tight money. The refusal of the economy to recover has been caused by some combination of the debt overhang from the 1980s together with a credit crunch caused by tighter regulation of bank balance sheets.

Theory

The second paper proposes a "migration model" to explain the seesaw response of real GNP changes to interest-rate changes, with

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a positive coefficient followed by a negative coefficient of about the same size. Before criticizing this model, let us reconsider the standard mainstream analysis based on the *IS–LM* model.[3] Consider any positive demand shock that moves *IS* to the right, say an increase in spending for the Vietnam War in 1966–68. The economy's equilibrium shifts to the northeast as *IS* slides up *LM*. We observe then the positive contemporaneous correlation between Δi and ΔGNP that Ranson observes, and we can allow for partially accommodating monetary policy as long as the Fed does not peg the interest rate completely.

After a while, however, the correlation shifts. The *LM* curve begins to move to the left, sliding the economy northwest along the *IS* curve. The leftward *LM* shift comes from a falling growth rate of real money, stemming from some combination of monetary tightening in response to the previous expansion, and the direct effect of higher prices in reducing real money. Ever since the early days of the MIT–Penn (MPS) model in the mid-1960s, we have been used to distinguishing three channels of monetary influence on the real economy: (1) the substitution effect as an increase in interest rates reduces the net return to investment activity; (2) the availability effect related to nominal interest-rate ceilings, also known as credit rationing and disintermediation; and (3) the wealth effect of changing stock and bond prices. All three channels contribute to the negative correlation between real GNP changes and lagged interest-rate changes.

As an alternative to this standard view, Ranson posits a convoluted mechanism based on substitution, which is already present in conventional channel (1), but in his version the usual disincentive effect of higher interest rates on investment is ruled out. What he requires is that higher interest rates are a *signal* of increased expected inflation, which in turn makes people *believe* they will be pushed into higher effective tax brackets in the future. His business cycle is based entirely on voluntary substitution between periods in response to expected future changes in effective tax rates. This channel of monetary influence strikes me as a second-order effect, likely to be a sideshow compared with the direct incentive, availability, and wealth effects. One reason we know that it must be a sideshow is that most year-to-year changes in nominal interest rates do not signal changes in future inflation, because most are accompanied by changes in *real* interest rates.

[3]Thus, I reject Ranson's claim that his "conclusions do not fit into the standard IS–LM framework in any obvious way" (Ranson 1992, p. 198).

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TABLE 3

QUARTERLY REGRESSIONS OF PERCENTAGE CHANGE IN REAL GNP ON LAGGED CHANGES IN NOMINAL AND REAL TREASURY BILL RATE

Time Period	Sum of Coefficients on Lagged Δi (Lags 2-9)	$\bar{R}^2$	S.E.E.	Forecast Error, Sample Ends 1990:3					
				1990:4	1991:1	1991:2	1991:3	1991:4	Mean Error
1955:1–1990:3	−1.69**	0.24	3.46	−3.46	−5.54	−3.98	−2.65	−4.32	−3.98
Subperiods									
1955:1–1969:4	−3.73**	0.23	3.32	−4.18	−6.81	−7.33	−7.85	−8.80	−6.99
1970:4–1982:4	−1.89**	0.20	4.22	−3.47	−5.89	−4.63	−3.35	−4.82	−4.43
1983:1–1990:3	−1.27**	0.37	2.03	−3.83	−5.72	−3.19	−1.52	−3.99	−3.65
Real interest rate, full period									
Without supply shocks	−1.40**	0.09	3.80	−4.35	−6.08	−4.00	−2.20	−3.87	−4.10
With supply shocks	−1.16**	0.15	3.67	−5.48	−6.34	−3.17	0.43	−1.63	−3.24

NOTE: All equations also contain a constant term, and those for the entire period and middle subperiod include a constant shift term for 1973–90. The final line enters lags 0–4 of the difference between the quarterly rate of change of the fixed-weight consumption deflator and the rate of change of the same deflator excluding food and energy. Statistical significance at the 5 percent level is indicated by * and at 1 percent by **.

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From all this I reach the conclusion that changes in short-term interest rates are normally a useful leading indicator, but not in 1990–91. The huge recent forecasting error means that we are more uncertain than ever about the multipliers of monetary policy and about the right dose of monetary ease needed to generate an economic recovery. To understand why (normally) real GNP changes are negatively correlated with changes in short-term interest rates over the previous nine quarters, we can do no better than to accept the wisdom of the MPS model as set out 25 years ago.

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Money Targeting and Interest-Rate Targeting in an Uncertain World

Eduard J. Bomhoff

Introduction

Each central bank needs to provide the economy with a nominal anchor for which it is responsible. There are three basic ways of fulfilling this task: monetary targeting, interest-rate targeting, or exchange-rate targeting. Of these three options, exchange-rate targeting means that the anchoring takes place on foreign soil. Currently, no worldwide arrangement of fixed exchange rates is under discussion, and in the North American region, the United States is so much larger than both Canada and Mexico that those countries may tie their currencies to the dollar. But the United States cannot anchor the dollar to the currency of one of its North American trading partners.

The Federal Reserve, therefore, has to choose between the two domestic methods of providing a nominal anchor to the economy: monetary targeting or interest-rate targeting. An omniscient central bank would be indifferent, because the methods are equivalent in terms of resource costs. A realistic central bank has to compare the informational requirements of both methods to decide which will work best in an uncertain world. The types of uncertainty that most affect the economy are crucial here, not because one method can react more flexibly to unforeseen developments, but because different economic surprises have different effects on the degree to which the central bank can attain its targets—depending on whether interest rates or monetary aggregates are used as intermediate targets and indicators of monetary policy.

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Economic analysis of the central bank's problem requires a model that describes (a) how inflationary expectations are formed and (b) how the central bank, through steering a monetary aggregate or an interest rate, can influence future price levels or rates of inflation. The basic relations needed for a simple model of monetary policy are, therefore, a demand-for-money schedule and the definition of the real rate of interest as the difference between the nominal rate and the expected rate of inflation. These two relations will be embedded in a stochastic structure that allows for transitory and permanent shocks to the real rate of interest, the income velocity of money, and the rate of monetary expansion.

In the second section of the paper, I describe one such model, that of Robert Barro (1989). His model allows for a limited number of shocks, postulates fully flexible prices, and implies that interest-rate targeting has benefits (and no costs) over money targeting. In the third section, I develop an alternative model that is similar to Barro's but has some degree of sluggishness in prices and a richer menu of stochastic shocks. In the fourth section, I perform stochastic simulations of the different models. Solving the so-called Riccati equations associated with the systems of equations gives a measure of success in conducting monetary policy that is conditional on the choice of instrument and on the importance of the different types of shocks.[1] Analytical solutions of Riccati equations are generally not feasible, so the results in the paper will be numerical and will depend on the inputs: the interest-rate elasticity of the demand for money, as well as the size and importance of a variety of shocks to money demand, money supply, and real interest rates.

The principal result of the simulations is that interest-rate targeting becomes inferior to monetary targeting as soon as the assumption of price flexibility is abandoned. Barro's results are seen to be quite model-specific. For a variety of assumptions regarding the importance of different types of shocks to the economy, I find that monetary targeting results in superior steering of the rate of inflation over time.

Barro's Model of Interest-Rate Targeting

There are three equations in Barro's (1989) model:

$$(1)\quad i_t = p^e_{t+1} - p_t + r_t + v_t$$

[1]Results also depend on the numerical value of the interest elasticity of the demand for money, but uncertainty regarding this number is much less troublesome than is often assumed, as long as the appropriate forecasting techniques are used.

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(The nominal interest rate equals the real rate, r_t, plus the expected rate of price change between this period and the next, $p^e_{t+1} - p_t$ plus an observation error, v_t.)

$$(2)\quad M_t - p_t = \alpha_t - \beta i_t + \omega_t$$

(The demand for money equals an intercept, α_t, a term in the interest rate, βi_t, and a residual, ω_t.)

$$(3)\quad i_t = i_{t-1} + u_t$$

(The central bank targets the nominal interest rate, shifting its target from period to period in random fashion.)

Barro shows in the context of his model that interest-rate targeting is always superior to monetary targeting. The central bank can target nominal interest rates as intensely as it wishes without endangering its control of the price level. There is no tradeoff between the two: Interest targeting is a free good.

This remarkable result depends on the following four strong assumptions. First, the current price level can adjust one-to-one to any current differences between the expected rate of growth in the money supply and the actual rate of growth of the money supply.[2] Second, the demand for money function contains just a single opportunity cost variable: the administered interest rate. The real rate of return implied in this administered interest rate is equal to the real rate of return that is exogenously given to the economy.

Third, the authorities manage to convey to the public in each period a signal of the intended growth of the nominal money supply between this period and the next. The public receives this signal without any type of noise and with full credibility in the central bank's intentions. Hence, the expected rate of inflation between this period and the next can be influenced by the authorities through variations in the expected rate of growth of the nominal money supply.[3]

Fourth, Barro evaluates monetary targeting and interest-rate targeting according to their success in minimizing the discrepancies between expected and actual price levels. This approach is consistent with his assumption that prices are perfectly flexible. However, if one takes another view of price stickiness, one would want to

[2]This assumption contrasts with the position in Barro (1983, p. 102) that the lag between changes in money and consequent changes in the price level takes up to four years.

[3]Barro views the expected rate of inflation as the difference between the actual price level this year and the expected value of next year's price level, rather than as some underlying, sluggish variable that is embedded in temporary and permanent shocks to the price level.

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compute the unconditional variance of the price level or the rate of inflation.

Given these four assumptions, the results in Barro's paper follow and do not depend on the relative importance and size of the various shock terms.

The assumption that a single interest-rate variable in the money demand function is appropriate for theoretical analysis of interest-rate targeting versus the targeting of a monetary aggregate has been common to the academic literature on this topic. Bennett McCallum (1981, p. 323) remarks: "Most analyses of the instrument problem have been conducted in models with only one interest rate." He argues that introduction of a second opportunity cost variable is not useful if one closes the model with a relation that connects these interest rates. In that case, any discrepancies between the administered and the market interest rate(s) would be "random and uninteresting."

McCallum's argument shows that he also assumes maximum flexibility of the price level. Only on that assumption would an arbitrary setting of the administered interest rate be followed immediately by all market interest rates. Under these extreme assumptions, the authorities can steer the evolution of the sum of the real rate of interest and the expected rate of inflation through their manipulation of the single relevant interest rate. If we use the common assumption in this type of literature that the real rate of interest is given for the analysis, the degree of accuracy with which the authorities are able to estimate the real rate will then determine the degree of accuracy in delivering a desired path for the price level.[4]

Monetary Policy in an Uncertain World

Karl Brunner and Allan Meltzer (1989) have always maintained that at least two rates of return on financial or physical assets are required for proper analysis of monetary policy, including the evaluation of interest-rate targeting versus money-supply targeting. Some essential features of such a two-interest-rate model are preserved in the following system of equations:

$$(4)\quad i_l = r + \Delta p^e + \sigma_t(r)$$

[4]If the real rate is not given and is, in fact, irrelevant because the model is limited to a feedback rule for the nominal interest rate and a money demand schedule, the price level may become undetermined as in the classical article by Thomas Sargent and Neil Wallace (1975).

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(The long-term interest rate, i_l, is equal to the real rate, r, plus the expected rate of inflation, Δp^e, plus a temporary error of observation, $\sigma_t(r)$.)

$$(5)\quad i_s = r + \Delta p^e + A$$

(The short-term interest rate, i, equals the long-term rate except the observation error plus or minus a term, A, which represents the current movement in the short-term rate to equilibrate the demand for money with its supply.)

$$(6)\quad p = p^e + \sigma_t(p)$$

(The current price level, p, equals the expected price level, p^e, plus a random distributed error, $\sigma_t(p)$.)

$$(7)\quad M - p = m - \epsilon i_l - \eta i_s + \sigma_t(m)$$

(The demand for real balances depends on an intercept, m, the long-term interest rate, the short-term interest rate, and a residual error, $\sigma_t(m)$.)

The dynamics of this simple system in which the rate of real output, y, has been omitted as nonessential, are governed by the following assumptions:

- The real rate of interest and the intercept in the demand for money equation are subject to permanent shocks in each period (as in Barro's model).
- The intercept in the demand for money schedule is subject also to permanent shifts in its rate of growth (in other words, the trend in velocity is stochastic and has to be deduced from observations on money and prices).
- Only the short-term interest rate can adjust to equilibrate the supply and demand for money.

In this alternative model, the actual price level is simply equal to the expected price level plus or minus a random observation error. This setup is obviously at variance with most theoretical analyses of interest-rate targeting, including Barro's, in which the current price level does adjust to current monetary shocks but conforms to the analysis in James MacKinnon and Ross Milbourne (1988), who document the dangers of using results from the money demand literature to mechanically derive price adjustment equations. Their paper convincingly shows that standard stories used to justify particular dynamics in the demand for money, such as the buffer stock approach, fail to provide explanations for the much longer lags between money growth and inflation.

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The alternative model can be used both to simulate money targeting and to study interest-rate targeting. Under an interest-rate rule, the adjustment needed to equate money supplied to money demanded in the current period is carried out by the money supply, which will differ from its expected value. Under monetary targeting, an opportunity cost variable has to adjust. Because all surprises within the current period have to do with temporary shocks, there is no need for the long-term interest rate to adjust. At the short end, the term A in equation (5) is available to equilibrate demand and supply of money (recall that the real rate of interest is exogenous, both in Barro's model and in the alternative model).

The degree of uncertainty about the expected rate of inflation, Δp^e, is now a function of the 10 variances that govern the dynamics of this alternative system. We shall look both at the conditional variance that measures the forecast errors in predicting inflation, and at the unconditional variance that indicates how far inflation will typically deviate from a constant path. Academic research on interest-rate targeting versus monetary targeting has concentrated on the conditional variance that can be computed analytically and has often neglected the unconditional variance that needs to be computed from the backward Riccati equation for each set of assumptions about the uncertain shocks that influence the economy.

The list of differences between Barro's model and the alternative model is presented in Table 1.

Simulation Results

In the simulations, values are inserted for the variances of the shock terms in the models that are derived from econometric computations using U.S. data. Some additional simulations use alternative values to see how sensitive the relative success of interest-rate targeting and money targeting are to these variance terms.

Uncertainties Regarding the Price Level[5]

I have estimated a univariate Kalman filter model for the logarithm of the U.S. GNP deflator over the 1948–88 period. The filter produces optimal estimates of the variances of the temporary and permanent shocks to the price level, and of the variance of the changes to the trend, respectively:

$$\sigma_t(p) = 0.414 * 10^{-4}$$
$$\sigma_p(p) = 0.0132 * 10^{-4}$$

[5]These estimates are not applicable to Barro's model.

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TABLE 1

DIFFERENCES BETWEEN BARRO'S MODEL AND THE ALTERNATIVE MODEL

Barro's Model	Alternative Model
1. Price level adjusts to shocks within the current period.	1. Short-term interest rate or money supply adjusts to current shocks.
2. Public receives money supply data without error.	2. All money growth numbers are noisy.
3. Inflation is defined as the difference between today's price level and the expected price level for next year.	3. Inflation is defined as underlying ("permanent") growth rate of the price index.
4. Success is measured by forecast errors in the price level.	4. Success is measured by the stability of the price level.
5. No stochastic trend is in velocity.	5. Stochastic trend is in velocity of money.
6. A single administered interest rate is in the demand for money; real rate computed from this interest rate equals "the" real interest rate.	6. Two interest rates are in the demand for money; real rate computed from the administered rate may differ from "true" real rate.

$$\sigma_{tr}(p) = 3.087 * 10^{-4}$$

$$\text{RMSE} = 0.022;\ \rho(1) = 0.304;\ Q(5) = 23.178;\ H(12) = 4.85;\ N = 0.266.$$

RMSE refers to the one-period-ahead forecast errors of the Kalman filter, $\rho(1)$ to the first-order coefficient of serial correlation of these errors, $Q(5)$ to the Box-Pierce test for serial correlation, H to the test for heteroskedasticity from A. C. Harvey (1989, p. 259), and N to a test for normality of the one-period-ahead forecast errors of the Kalman filter (see Harvey 1989, p. 260). I shall use the estimated variance of the temporary shocks as a measure of the temporary observation errors in the price level in the alternative model.

Uncertainties Regarding the Money Supply[6]

Univariate models for the U.S. money supply M1 and M2 for the 1948–88 period resulted in the following values:

[6]Ibid.

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$$\sigma_t(M1) = 0.0374 * 10^{-4}$$
$$\sigma_p(M1) = 5.198 * 10^{-4}$$
$$\sigma_{tr}(M1) = 0.456 * 10^{-4}$$

RMSE = 0.026; $\rho(1) = 0.044$; $Q(5) = 5.045$; $H(12) = 2.981$; $N = 1.842$.

$$\sigma_t(M2) = 0.0118 * 10^{-4}$$
$$\sigma_p(M2) = 7.534 * 10^{-4}$$
$$\sigma_{tr}(M2) = 0.506 * 10^{-4}$$

RMSE = 0.029; $\rho(1) = 0.280$; $Q(5) = 6.728$; $H(12) = 1.944$; $N = 1.122$.

Here and below, the three variances again refer to the temporary and permanent shocks to the level and to the increments in the stochastic trend, respectively.

In order not to deviate too much from Barro's model, which does not incorporate any uncertainty about rates of growth, I have assumed that the planned rate of growth of money is observed without error by the economic agents. Later, an experiment will be performed that does include uncertainty about the expected rate of growth of money.

Uncertainties Regarding the Intercept in the Demand for Money Schedule (Shocks to Velocity)

I have estimated a single-equation Kalman filter model for U.S. M1 and M2, regressing the inverse of velocity on a stochastic intercept and on the logarithm of the long-term government bond rate (see Bomhoff 1991). Results are as follows:

M1: $\sigma_t(m) = 0.021 * 10^{-4}$
$\sigma_p(m) = 13.970 * 10^{-4}$
$\sigma_{tr}(m) = 0.0113 * 10^{-4}$

RMSE = 0.035; $\rho(1) = 0.338$; $Q(5) = 5.353$; $H(12) = 4.547$; $N = 2.742$.

M2: $\sigma_t(m) = 0.224 * 10^{-4}$
$\sigma_p(m) = 0.104 * 10^{-4}$
$\sigma_{tr}(m) = 16.828 * 10^{-4}$

RMSE = 0.040; $\rho(1) = 0.009$; $Q(5) = 9.651$; $H(12) = 2.823$; $N = 0.899$.

Note that Barro allows only for shocks to the level of velocity, whereas the alternative model also has changes to the trend in the income velocity of money.[7]

[7]For a thorough analysis of the evidence in favor of a stochastic trend in velocity, see Bordo and Jonung (1987).

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Uncertainties Regarding the Real Rate of Interest

When I used the computed values for the expected rate of inflation from the model for the price level, subtraction from the long-term U.S. government bond rate produced a time series for the real rate of interest to which a second Kalman filter was applied as follows:

$$\sigma_t(r) = 0.0045 * 10^{-6}$$
$$\sigma_p(r) = 2.0151 * 10^{-4}$$
$$\sigma_{tr}(r) = 0.0016 * 10^{-4}$$

$$\text{RMSE} = 0.014;\ \rho(1) = 0.346;\ Q(5) = 10.123;$$
$$H(12) = 31.215;\ N = 12.194.$$

The variances for the shocks to the price level, the money supply, and the real rate of interest having been computed from univariate models, these numbers should be regarded as upper bounds rather than optimal estimates. Improved estimation of these variances is a topic for future research.

The solutions to the models assume that agents are aware of the relative importance of the disturbances to velocity, money, and real interest rates. The solutions agree on the specification of the model, including the numerical value of the interest-rate elasticity of the demand for money. The interest elasticity has been set at 0.043 for M1 and 0.049 for M2, both on the basis of the Kalman filter estimates for the velocity function.

Numerical solution of the forward Riccati equations for the Kalman filter system results in equilibrium values for the variances of the state variables in Barro's model.[8] Since the expected price level is one of the state variables, this procedure answers a question traditionally posed in the theoretical literature on money targeting versus interest-rate targeting: How do the conditional forecasts of the price level compare under various regimes? However, this way of formulating the problem makes sense only if the path of the expected price level and the expected rate of inflation are of no concern. If they are, one would have to compute the variation in the price level around some desired path.

[8]Barro makes two simplications to his model so he can use the method of undetermined coefficients to produce an analytic solution: First, the real rate of interest is observed without error at a one-year delay, and second, any permanent shifts to the demand for money are also known precisely with a one-year delay. Even though such simplifications are not needed in the Kalman filter approach, I have incorporated them in order to perform simulations that are fully applicable to Barro's analysis.

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The first lines in both Table 2 and Table 3 relate to the conditional variance of the current price level under interest-rate targeting or money targeting. They show that forecast errors of the price level in Barro's model do not depend on whether the authorities follow a money rule or an interest-rate rule. This finding confirms Barro's principal result that interest-rate targeting delivers control over interest rates with no cost in the form of higher forecast errors in inflation.

TABLE 2

FORECAST ERRORS IN BARRO'S MODEL

M1	Interest-Rate Targeting	Money Targeting
Var($p_t - E_{t-1}p_t$)	$14.38 * 10^{-4}$	$14.38 * 10^{-4}$
Min [var $E_{t-1}p_t$]	$28.36 * 10^{-4}$	$28.36 * 10^{-4}$
Min [var $E_{t-1}p_t$ + varΔi_t]	$52.56 * 10^{-4}$	$52.56 * 10^{-4}$
(of which var($E_{t-1}p_t$)	$32.51 * 10^{-4}$	$32.51 * 10^{-4}$)
M2	**Interest-Rate Targeting**	**Money Targeting**
Var($p_t - E_{t-1}p_t$)	$0.52 * 10^{-4}$	$0.52 * 10^{-4}$
Min [var $E_{t-1}p_t$]	$0.63 * 10^{-4}$	$0.63 * 10^{-4}$
Min [var $E_{t-1}p_t$ + varΔi_t]	$2.62 * 10^{-4}$	$2.62 * 10^{-4}$
(of which var($E_{t-1}p_t$)	$0.66 * 10^{-4}$	$0.66 * 10^{-4}$)

TABLE 3

FORECAST ERRORS IN THE EXTENDED KALMAN FILTER MODEL

M1	Interest-Rate Targeting	Money Targeting
Var($p_t - E_{t-1}p_t$)	$0.80 * 10^{-4}$	$0.46 * 10^{-4}$
Min [var $E_{t-1}p_t$]	$38.79 * 10^{-4}$	$38.79 * 10^{-4}$
Min [var $E_{t-1}p_t$ + varΔi_t]	$51.36 * 10^{-4}$	$51.36 * 10^{-4}$
(of which var($E_{t-1}p_t$)	$44.50 * 10^{-4}$	$44.50 * 10^{-4}$)
M2	**Interest-Rate Targeting**	**Money Targeting**
Var($p_t - E_{t-1}p_t$)	$0.80 * 10^{-4}$	$0.55 * 10^{-4}$
Min [var $E_{t-1}p_t$]	$32.55 * 10^{-4}$	$32.55 * 10^{-4}$
Min [var $E_{t-1}p_t$ + varΔi_t]	$95.21 * 10^{-4}$	$95.21 * 10^{-4}$
(of which var($E_{t-1}p_t$)	$46.29 * 10^{-4}$	$46.29 * 10^{-4}$)

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The remaining lines in Tables 2 and 3 refer to unconditional variances under different monetary policy arrangements. In other words, the numbers indicate the average (squared) error margins if one tried to maintain the indicated variable(s) on some predetermined path. These second moments do not depend on the choice between targeting the money supply and letting the interest rate absorb shocks to the demand for money, or setting an interest rate instrument and allowing the money supply to equilibrate the money market. The rule setting the desired (expected) rate of growth in money is identical for M1 and M2. That rule depends only on the state update equations of the Kalman filter and on the cost-objective function that is being minimized by the central bank.

If the authorities are interested only in stabilizing the price level, seeking formally

$$(8)\quad Min\ E\{\frac{1}{2}\sum_{t=0}^{\infty}(p^e_t)^2\},$$

the resulting optimal rule for the expected growth of money becomes

$$(9)\quad \Delta M^e_{t+1} = -p^e_{t+1}.$$

Simulation results are shown in lines 2 of Table 2.

In my alternative model, the optimal policy rule will be

$$(10)\quad \Delta M^e_{t+1} = -p^e_{t+1} - \Delta p^e_{t+1}.$$

Results are shown in lines 2 of Table 3.

If the central bank is also concerned about changes in interest rates and seeks the following policy objective

$$(11)\quad Min\ E\{\frac{1}{2}\sum_{t=0}^{\infty}((p^e_t)^2 + B(\Delta i_t)^2)\}$$

with

$$(12)\quad \Delta i_t = \Delta(r_t + p^e_{t+1} - p^e_t)$$

with B a constant indicating the relative importance of price-level stabilization and interest-rate smoothing, the optimal policy rule in Barro's model for the arbitrary choice $B = 1$ will be

$$(13)\quad \Delta M^e_{t+1} = -0.2496\, p^e_{t+1} - 0.2309\, p^e_t.$$

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Results are shown in lines 3 of Table 2.[9] The last lines of Table 2 show to what extent the steering of the price level becomes less accurate if the central bank is concerned also with interest-rate smoothing.

In my model, the optimal rule for the value $B = 1$ becomes

$$(14)\quad \Delta M^e_{t+1} = -0.4805\, p^e_{t+1} - 0.7691\, \Delta p^e_{t+1}.$$

Results are shown in Table 3, with the bottom lines indicating the deterioration in the quality of the price-level stabilization.

Barro makes his case for interest-rate targeting through proving that keeping the sole (administered) interest rate on a predetermined path does not cause a deterioration in the forecast errors of inflation, given the assumptions of his model. Tables 2 and 3 show how the *steering* of inflation, as opposed to its *forecasting*, does suffer if one tries to minimize changes in an interest rate as well. Interest-rate targeting is no longer a free good, if we consider the unconditional rather than the conditional variance of the price level.

Tables 4 and 5 show the effects of changes in some variance terms on the computed uncertainties in the price level. In the standard simulation, monetary targeting is more attractive than interest-rate targeting if one considers the conditional variances. As far as the unconditional variances are concerned, the relevant point is the difference between lines 2 and 3, which indicate the cost if one wishes to steer the price level and smooth interest rates at the same time. In the standard simulation, the unconditional variance of the price level increases by half if the authorities intend to smooth interest rates.

Columns 2 through 5 of Tables 4 and 5 show results for four alternative simulations: Column 2 adds uncertainty about the expected rate of growth of the money supply according to the estimated models for U.S. M1 and M2, column 3 increases the uncertainty about the permanent shifts in the real rate of interest by a factor of four, and columns 4 and 5 give results for models with greater uncertainty about the trend in the intercept of the demand for money function (the trend in velocity). Note that in all cases the assumption that the central bank is interested in interest-rate smoothing has a cost in the form of greater discrepancies between planned and actual paths of the price level.

[9]Because real rates of interest are assumed to be nonstationary in Barro's model, it is not feasible to try to control both the path of both the price level (or the rate of inflation) and the interest rate. Hence, the closest control-theoretic substitute to Barro's analysis of forecast errors is to minimize some combination of the squared deviations from a given *path* for the price level and the squared forecast errors in interest rates.

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TABLE 4

GREATER UNCERTAINTY IN THE EXTENDED MODEL

	Interest-Rate Targeting				
	(1)	(2)	(3)	(4)	(5)
$\text{Var}(p_t - E_{t-1}p_t)$	0.80	0.80	0.80	0.80	0.80
Min [var $E_{t-1}p_t$]	38.79	40.61	39.15	38.82	39.90
Min [var $E_{t-1}p_t$ + varΔi_t]	51.36	54.73	58.52	52.51	57.20
(of which var$(E_{t-1}p_t)$	44.50	46.60	44.60	44.56	46.38)
	Money Targeting				
	(1)	(2)	(3)	(4)	(5)
$\text{Var}(p_t - E_{t-1}p_t)$	0.46	0.46	0.46	0.46	0.46
Min [var $E_{t-1}p_t$]	38.79	40.61	39.15	38.82	39.90
Min [var $E_{t-1}p_t$ + varΔi_t]	51.36	54.73	58.52	52.51	57.20
(of which var$(E_{t-1}p_t)$	44.50	46.60	44.60	44.56	46.38)

(1) Standard solution.
(2) Added: $\sigma_u(M) = 0.456 * 10^{-4}$.
(3) Change: $\sigma_p(r)$ multiplied by 4 to give $8.060 * 10^{-4}$.
(4) Change: $\sigma_u(m)$ multiplied by 4 to give 0.0452.
(5) Change: $\sigma_u(m)$ multiplied by 100 to give 1.13.
NOTE: All entries must be multiplied by 10^{-4}.

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TABLE 5

GREATER UNCERTAINTY IN THE EXTENDED MODEL (M2)

	Interest-Rate Targeting				
	(1)	(2)	(3)	(4)	(5)
Var($p_t - E_{t-1}p_t$)	0.80	0.80	0.80	0.80	0.80
Min [var $E_{t-1}p_t$]	32.55	32.58	32.67	83.04	1698.5
Min [var $E_{t-1}p_t$ + varΔi_t]	95.21	97.85	101.41	313.84	7309.9
(of which var($E_{t-1}p_t$)	46.29	48.62	46.42	131.16	2846.85
	Money Targeting				
	(1)	(2)	(3)	(4)	(5)
Var($p_t - E_{t-1}p_t$)	0.55	0.55	0.56	0.55	0.55
Min [var $E_{t-1}p_t$]	32.55	32.58	32.67	83.04	1698.5
Min [var $E_{t-1}p_t$ + varΔi_t]	95.21	97.85	101.41	313.84	7309.9
(of which var($E_{t-1}p_t$)	46.29	48.62	46.42	131.16	2846.85

(1) Standard solution.
(2) Added: $\sigma_{tr}(M) = 0.506 * 10^{-4}$.
(3) Change: $\sigma_p(r)$ multiplied by 4 to give $8.060 * 10^{-4}$.
(4) Change: $\sigma_{tr}(m)$ multiplied by 4 to give $67.312 * 10^{-4}$.
(5) Change: $\sigma_{tr}(m)$ multiplied by 100 to give $1682.79 * 10^{-4}$.
NOTE: All entries must be multiplied by 10^{-4}.

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Conclusion

This paper uses models of money demand and money supply to investigate the differences between monetary targeting and interest-rate targeting. The main finding is that one needs to differentiate between two meanings of the term interest-rate targeting. In his influential paper, Barro (1989) used the term both to indicate the choice of an interest rate as instrument of monetary control and to refer to a central bank objective that does include the smoothing of interest rates. In my paper, I separate the two different meanings of interest-rate targeting both in Barro's own model and in an alternative model that allows for a richer menu of shocks to the economy and for less-than-perfect flexibility in the national price level.

In models that allow for a wider variety of shocks than Barro's model, his specific result that the conditional variance of the price level does not depend on whether an interest-rate target or a monetary aggregate target is used will no longer hold. More important, however, are the results, both in Barro's model and in the alternative model for the unconditional variances of the price level. If *expected* rates of price change also carry a social cost, then the central bank cannot limit itself to minimizing forecast errors in inflation, as in Barro's paper.

Neither in Barro's model nor in my alternative model is it the case that the central bank can add interest-rate smoothing to its objective function without paying a cost in terms of lower accuracy in its price-level policies. There are significant differences in the degree of control, depending on whether the central bank minimizes

$$(15)\quad Min\ E\{\frac{1}{2}\sum_{t=0}^{\infty}(p^e{}_t)^2\}$$

or

$$(16)\quad Min\ E\{\frac{1}{2}\sum_{t=0}^{\infty}((p^e{}_t)^2 + B\,(\Delta i_t)^2)\}$$

with B as a constant. This result holds, irrespective of whether a monetary aggregate or an interest rate is used as the short-term instrument of monetary control. Hence, it appears that analysis of monetary policy should distinguish between "interest-rate targeting," in the sense of using an interest rate as the short-run policy instrument, and "interest-rate smoothing," meaning the incorporation of some function of interest rates in the objective function. The former may be attractive depending on the information structure of the economy, but it does not follow that the latter—smoothing of interest rates—

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can be achieved without paying the cost of less success in the central bank's prime task: the stabilization of the price level.

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Setting a Framework for Monetary Policy

Alan C. Stockman

Eduard Bomhoff's paper is a hands-on attempt to work toward improving policy rules. This kind of research is exceedingly important. I have some quarrels with specific details of Bomhoff's model, his assumptions, and the way he chooses to simulate it, and I will outline my objections here. But I also want to use the general framework behind Bomhoff's research to discuss its implications for the basic question of whether we should try to construct better forecasting models within a discretionary policy regime or try to improve the policy rules.

Bomhoff's paper extends previous work by Robert Barro on the relative merits of money-supply targeting and interest-rate targeting. In particular, he extends the analysis to include sticky prices, an endogenous real interest rate that can be affected by monetary policy, and multiple interest rates (long-term and short-term rates) in the demand for money function (as suggested by the work of Karl Brunner and Allan Meltzer). He incorporates estimates of the sizes of real-life shocks into his model and examines their implications. Bomhoff concludes, in contrast to Barro, that interest-rate targeting raises the variance of forecast errors of inflation above the variance that would be obtained under money-supply targeting. He also finds that the (unconditional) variance of inflation is higher under interest-rate targeting than under money-supply targeting.

Comparing different policy rules and extending models to incorporate realistic features that might affect the conclusions are important. It is also important to evaluate alternative policy rules under a variety of models, since economists disagree about which model is "best."

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I have several specific objections to the analysis in Bomhoff's paper, however.

Objections to Bomhoff's Analysis

First, the model is not complete; it leaves unexplained certain key assumptions. There are two interest rates in the money demand function: a short rate and a long rate. I have no serious objection to this. But the paper assumes that only the short-term interest rate (and not the long rate) responds in the short run to economic disturbances. This is a strange assumption: It is well known that long-term interest rates often change rapidly. In fact, many economists regard it as a puzzle that long-term interest rates do not remain more stable in the short run.[1]

It is common in the literature to link short-term rates and long-term rates through some theory of the term structure, such as the expectations theory (or some extension that involves liquidity premia on long rates). Any such theory implies some relation between long-term and short-term interest rates, reflecting arbitrage and speculation in financial markets. Bomhoff assumes that any such relation is broken in the short run by shocks that affect the supply or demand for money. That is the source of the arbitrary term A in equation (5), which Bomhoff assumes moves to equilibrate the money market in the short run. But he does not include a theory of why the relations generated in financial markets between returns on short-term and long-term interest-bearing assets should be affected in this way. One might expect, instead, that the entire term structure of interest rates rises and falls to equilibrate the money market in the short run.[2] That is, interest rates may adjust to equilibrate the money market, but the relations between various interest rates would be unaffected. Just as we expect short-term interest rates on relatively riskless assets (such as U.S. treasury bills) and more risky assets (such as low-grade commercial paper) to be related by a risk premium that is roughly unaffected by shocks to the money market, so we should also expect interest rates on assets of various terms to be related by term premia that are roughly unaffected by shocks to the money market. How would Bomhoff's results differ if he eliminated his assumption that long-term rates remain constant (so that term premia would remain

[1]For example, many studies a few years ago found large responses of long-term rates (as well as short rates) to money-supply announcements.

[2]That idea would appear to be more consistent with the money-announcement results mentioned in footnote 1.

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stable)? I do not know the answer, but it would be worth studying, at least subject to my second objection.

Bomhoff's model assumes that the economy is completely isolated from events in foreign countries. In particular, there are no international financial markets in the model. So there are no connections between home and foreign interest rates. This could be a serious omission, because world financial markets create connections between interest rates in different countries. The links between these rates may not be tight, because there are information and transactions costs of arbitraging these markets. But even loose links create connections between these rates. In particular, the world real interest rate places limits on the size of interest-rate changes in any one country. Those limits are missing from Bomhoff's model.

To some extent, the differential between U.S. and foreign interest rates may change to equilibrate the U.S. money market in the short run (as Bomhoff's model implicitly assumes.) But because world financial markets place limits on arbitrary changes in this differential, one of two other things must happen. Either world interest rates must change to equilibrate the U.S. money market or something else must change to equilibrate that market. (A third alternative is that nothing equates the money market in the short run; disturbances to supply or demand in that market leave people with more or less real money than they would like to hold for a short period of time.) If world (and U.S.) interest rates change to equilibrate the U.S. money market, then they also change to help equilibrate money markets in other countries as well. Interest rates in all countries would then respond to world money-market conditions rather than conditions only in the United States. This would suggest an entirely different model than the one Bomhoff analyzes in his paper. It also raises a number of questions that economists have not yet satisfactorily answered. For instance, to what extent do money supply (or demand) changes in any one country affect the short-run differential between interest rates in that country and interest rates in other countries? Why do investors in world financial markets not arbitrage away any short-term changes in interest-rate differentials (adjusted for exchange-rate changes, of course) across countries? Can the Fed affect the world real interest rate? There has been a large literature on these questions, but existing studies have not yet produced reliable answers.

My third objection is related to the objective function that Bomhoff considers in conjunction with his assumption of sticky prices. In a world of sticky prices, monetary policy can have real effects on the economy—on real output and employment as well as on real interest

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rates. Since Bomhoff holds that the price level is sticky, we should care about the level of real output (and perhaps its variance) relative to the natural level of output, and not just inflation (or its variance) and nominal interest rates. It would be surprising if political forces led central banks to ignore the real output effects of their policies in such a world. And there is much evidence from real life that central banks pay considerable attention to what they regard as the real output effects of their policies (whether these effects are real or imaginary), particularly in countries with lower degrees of "independence" of central banks from the political process.[3]

Fourth, it is not clear that it makes sense to use the unconditional variance of the price level to evaluate policy. If a policy makes the price level nonstationary, then the variance of the price level does not exist. For this reason, Bomhoff uses the variance of the inflation rate (the rate of change of the price level). Now consider two policies. Policy A creates a stable long-run price level, so that any increase in the price level is eventually followed by a decrease (in technical terms, the price level is I(0)), but with some erratic short-run movements in prices. Now consider policy B, which produces a long-run inflation rate of 10 percent per year (the price level is I(1)), but which is very stable over time. If we compare the (conditional or unconditional) variance of the inflation rate or its predictability, we might find that the variance is lower under policy B. But is policy B better than policy A? Not necessarily: It seems quite reasonable to me to prefer policy A. Policy A allows people to sign long-term contracts in nominal terms that more closely approximate real contracts, because there is no long-run price-level drift. Fixed-rate mortgages under policy A would create inflation-adjusted mortgage payments that tend to remain flat over time; policy B would make inflation-adjusted payments start high and decline over time. So policy A has some clear advantages over policy B, but Bomhoff's criteria might favor policy B.

The key question is why we want "stable money," and exactly what we mean by that term. It could mean a stable level of prices, a stable inflation rate, or a stable expected inflation rate (but with forecast errors creating nonstationarity in the price level). Or perhaps it is a code for stable real GNP or other things. The answer is important because it affects the way economists should evaluate alternative

[3]As a related matter, Bomhoff does not consider the possibility of a policy of targeting a price level. Yet, with his criterion, such a policy would be a natural thing for the central bank to try to do. Below, I will discuss the merits of a target zone for the price level, which is a modified version of price-level targeting.

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monetary rules and alternative monetary institutions. Arbitrary measures such as variances of inflation or forecast errors, and other similar measures, may tell us little about the benefits and costs of alternative monetary rules. And they may shed little light on the debate between proponents of rules and proponents of better discretionary policies.

Rules versus Discretion

While studies like Bomhoff's can contribute to our understanding of the effects of various monetary rules, there are always economists and policymakers who advance serious arguments in favor of discretionary policy. (By serious arguments, I mean arguments that we should take seriously.) The key argument for discretion is this: No rule can cover all unforeseen events and complications. Discretion is not perfect, but it can respond to surprises and complications.

It might actually be true that a policymaker with some discretion can do better than the monetary rules that economists have proposed, just as economic forecasters can often do better by adding discretionary "fudge factors" (based on their nonquantifiable judgments) to their formal statistical forecasts. Unfortunately, the debate on rules versus discretion often gets reduced to a debate in which one side favors rules without discretion and the other side favors discretion without rules.[4]

Most participants in these debates have missed the important point: The case for rules does not rest on the notion (though it may be true) that discretion is worse than rules. Rather, the case for rules rests on the inability of discretionary policymakers to keep things from getting out of hand.[5] Imagine a dog in a yard. It may be wise to give the dog some discretion on where to walk or run, so some discretion may be the best institutional arrangement. But if the dog is without a leash, he may run into the street and get killed. (The dog may lack an ability to commit himself not to go in the street in the face of some overwhelming temptation.) So although some discretion is good, a rule (a leash) is also good if the rule (leash) prevents the dog's running from getting out of hand (into the street).

The case for a rule is like the case for a leash. A leash does not prevent discretion; it merely limits discretion. It keeps the results

[4]It is sometimes said that in a complicated world, it is too hard to conduct good discretionary policy. But this issue cuts both ways—it is also harder to formulate good monetary rules. If the economy is subject to a great deal of uncertainty, there may be more times when simple rules limit policy in undesirable ways.

[5]There is also a well-known case for rules based on allowing policymakers to commit to future courses of action. The ability to commit expands the set of available options for policy, which can lead to better results.

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of discretion from getting out of hand. The case for a commodity money, for example, is not that it produces optimal results in every situation, but that a commodity money prevents inflation from getting out of hand through political abuse of the supply of fiat money.

A Target Zone for the Price Level

In recent discussions of open-economy macroeconomic policy, there has been much discussion of "target zones" for exchange rates.[6] Whatever the merits of those proposals, there is an important lesson that applies here: A "target zone" for the price level can combine the most important elements of discretion with the most important elements of rules.

A target zone for the price level would include two parts: (1) upper and lower bounds for the price level and (2) a rule that goes into effect if the price level ever exceeds those bounds, and forces the central bank to follow policies that return the price level to the bounds over some period of time in that case.[7] The target zone is like a leash—it allows the central bank to use discretionary policies *as long as those policies do not allow inflation to get out of hand,* in the sense that the price level does not deviate too far from its target value. In this way, a target zone for the price level combines the most important benefits of discretion with the most important benefits of rules. A fixed target zone for the price level would create zero inflation in the long run (along with low nominal interest rates and the stability of long-run nominal planning) with the policy flexibility of discretion. If the central bank allows the price level to rise above the upper bound, the rule requires the central bank to follow a policy that slowly but surely guarantees that the price level will return to that target zone (perhaps within a couple of years, depending on the details of the proposal).

With the combination of rules and discretion afforded by a target zone for the price level, economists would want to continue the debate on the relative merits of discretionary policy within the target zone or further rules to restrict policy within that zone. Then the two rules that Bomhoff discusses—money-supply targeting and interest-rate targeting—could be evaluated within a framework in which the long-run inflation rate is zero with either policy. By adopting a target zone for the price level, we can fulfill the most important objectives of rules without taking a stand on the benefits or costs of short-

[6]I am not a fan of most target-zone proposals, but the application discussed below is somewhat different.

[7]For an example of this rule, see Gavin and Stockman (1992).

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run stabilization policies, money-supply targeting, or interest-rate targeting. We can achieve the main goals sought by advocates on each side of the debate.

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Making Monetary Policy

Robert L. Hetzel

This paper describes the procedures the Federal Open Market Committee (FOMC) of the Federal Reserve System follows in formulating monetary policy. It then examines Fed policy actions over the period 1986 through summer 1990. The rate smoothing and monetary deceleration that have preceded past recessions preceded the 1990 recession.

FOMC Procedures

Objectives

The FOMC employs judgmental procedures in formulating monetary policy. That is, the FOMC does not make use of an analytical framework for decisionmaking whereby it specifies explicit objectives and an explicit strategy for ensuring that each meeting's policy actions are consistent with achieving its objectives. This paper constructs an analytical model of the FOMC's judgmental procedures. Such a description must make inferences about the objectives that matter to the FOMC and about the model that expresses the FOMC's view of the links between those objectives and its policy actions.

Statements by the FOMC consistently mention qualitative objectives for inflation and real growth. The FOMC would like "sustainable" real growth and it would like to "make progress" in moving toward price stability. Such statements are usually uninformative about the relative emphasis the FOMC places on achieving each objective. For example, when the FOMC emphasizes reducing inflation, it is not explicit about the acceptable behavior of real growth.

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Because the Fed does not formulate explicit objectives, an analytical model of monetary policy must infer them. Such a model must also infer what the FOMC believes about how the structure of the economy constrains its ability to achieve its objectives.

Finally, because the FOMC does not make its objectives explicit, the interpretation of changes in its policy instrument, the funds rate, is ambiguous. In its discussions, the FOMC does not distinguish between (a) changes in the funds rate undertaken to *eliminate a discrepancy* between actual real growth (or inflation) and its implicit objective for real growth (or inflation) and (b) changes in the funds rate undertaken to *alter its objective* for real growth (or inflation). An analytical description of monetary policy must distinguish between changes in the funds rate undertaken to achieve the FOMC's ongoing objectives and changes undertaken to alter these objectives.

FOMC Judgmental Procedures

The following provides an overview of the monetary policy process as seen through the FOMC's "Record of Policy Actions." In the first half of their deliberations, FOMC members present their views about the economy. This "go-around" on the economy produces a majority view on the current and near-term behavior of real growth and inflation. It also produces a majority view on objectives. FOMC members never mention explicit numbers for their real growth and inflation objectives. Instead, they express their preferences about the relative importance to attach to these objectives by emphasizing undesirable behavior in either real growth or inflation. Through this emphasis, they attempt to influence the majority view on the importance to assign to either promoting real growth or reducing inflation.

In the second half of the meeting, FOMC members formulate the directive, which instructs the manager of the System Open Market Account (the trading desk) either to maintain the existing funds rate or to move it moderately up or down from its prevailing value. (At times, the FOMC targets the funds rate indirectly by setting a target for borrowed reserves, which introduces a small amount of random variation in the funds rate around its underlying target.) The FOMC's majority view on whether to stimulate real growth or reduce inflation determines how the FOMC moves the funds. Also, the FOMC imposes considerable continuity on the direction of changes in the funds rate. Over extended periods, it either ratchets the funds rate up or down.

In their discussions, FOMC members never refer to a model of the economy that would allow them to set the funds rate by working backward from objectives. FOMC discussions and public statements

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by the chairman, however, make clear that FOMC members believe that sustained changes in the funds rate will alter the growth of the public's nominal (dollar) expenditure. FOMC members also believe that alterations in the growth of nominal expenditure exert an impact on real growth initially, and only later on inflation. For example, in discussing a transition to price stability, Chairman Greenspan (1989c, p. 797) stated:

> During this transition period, growth would be reduced for a while from what it otherwise would have been. Because price-setting behavior in our economy has considerable momentum, the requisite slowing of demand would tend to translate, in the first instance, into a slowing of real output and only subsequently into restraint on prices.

An Analytical Description of FOMC Procedures

An analytical model of policy formulation relates objectives to intermediate targets and policy instruments. For the FOMC, the objectives are real output growth and the inflation rate. As noted above, the FOMC sets its economic priorities through informal discussion of the economy. Given its priorities for influencing real growth and inflation, the FOMC sets implicit targets for these objective variables as modest alterations from their perceived prevailing growth rates.

The FOMC uses the rate of growth of nominal GNP (the dollar value of gross national product) as an intermediate target. It lowers the funds rate moderately below the prevailing value if its priority is raising real output growth. Conversely, it raises the funds rate moderately above the prevailing value if its priority is lowering inflation. Although these changes in the funds rate are moderate in magnitude, they are sustained in the same direction until they alter the rate of growth of nominal GNP. The FOMC assumes that alteration in nominal GNP growth will move real output growth and inflation in the desired direction. The implicit target path the FOMC assigns to real output growth and inflation is given by its assumption that changes in nominal GNP growth impact initially on output growth, rather than inflation.

Lean against the Wind

To make the above discussion useful for understanding how the FOMC changes the funds rate, we must make two assumptions about the economy. Economists call the first of these assumptions the natural rate hypothesis. Specifically, we assume that the Fed can affect the real (inflation-adjusted) rate of interest only transitorily through

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unanticipated changes in money. The FOMC must have procedures that allow it (at least most of the time) to track the economy's underlying equilibrium interest rate. If it did not, if it set the funds rate arbitrarily relative to the economy's equilibrium interest rate, the economy would always lurch between inflation and deflation. We also assume that fluctuations in aggregate output arise more often from disturbances to the amount of goods the public attempts to acquire, rather than the amount of goods it attempts to supply.

The Fed's own description of the way it changes the funds rate is summarized in the phrase "lean against the wind." That is, the FOMC raises the funds rate when economic activity strengthens and lowers it when economic activity weakens. How do these procedures produce a funds rate compatible with the constraints imposed by the natural rate hypothesis? When economic activity strengthens because the public attempts to acquire more goods, the equilibrium real rate of interest must rise. The price of goods today in terms of goods tomorrow must become more expensive in order to restrict aggregate demand to aggregate supply. The Fed's lean-against-the-wind procedures, which cause it to raise the funds rate as real economic activity strengthens, produce the required rise in interest rates.

Changes in the funds rate that track the economy's underlying equilibrium interest rate will be called "policy maintaining." Such changes are like regular housekeeping chores. They are necessary because the economy's underlying equilibrium interest rate changes. Although the funds rate changes, monetary policy is unchanged. For example, a policy-maintaining change in the funds rate occurs when the FOMC raises the funds rate in response to an observed increase in economic activity. This funds-rate increase is policy maintaining in the sense that it keeps nominal GNP growing at its prevailing rate. Changes in the funds rate that alter the existing rate of growth of nominal GNP will be called "policy altering." For example, the FOMC makes a policy-altering change if it lowers the funds rate to increase the rate of growth of GNP.

As noted above, the FOMC makes changes in the funds rate in small but persistent steps. It uses the reaction of the bond market as a measure of when the individual changes have cumulated to a change large enough to achieve the desired effect on the rate of growth of nominal GNP. For example, if the FOMC is in the process of lowering the funds rate in response to weakness in economic activity and bond rates rise, the FOMC backs off, at least temporarily, from further decreases.

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Under the assumption that fluctuations in output derive primarily from fluctuations in aggregate demand so that strength in economic activity requires a higher interest rate, policy-maintaining changes in the funds rate *follow* changes in the rate of growth of real GNP. Policy-altering changes in the funds rate arise when changes in the funds rate *lag* behind changes in the growth rate of real GNP. Beginning in the early 1960s, the FOMC imparted inertia to the funds rate by raising it only well into periods of economic recovery. Similarly, it imparted inertia to the funds rate by lowering it only well into periods of economic weakness. As a consequence of this inertia, the behavior of the funds rate from the early 1960s through the early 1980s was primarily policy altering. To identify policy-maintaining behavior of the funds rate, we must concentrate on the period following the early 1980s.

Figure 1 shows predictions, available as of FOMC meetings, of nominal GNP growth for the quarter in which the FOMC meeting falls. Figure 1 also shows the initial value of the funds rate after an FOMC meeting.[1] (There were 11 FOMC meetings in 1980 and 8 per year thereafter.) In period 1, February 1980 through February 1981, the behavior of the funds rate and nominal GNP is dominated by the common influence of the Carter credit controls, imposed in March 1980 and removed in July 1980. In period 2, March 1981 through May 1982, changes in the funds rate are policy altering. Although predicted nominal GNP growth falls sharply, the funds rate rises initially. It then falls, but rises again in spring 1982. Over the entire period, the decline in nominal GNP growth greatly exceeds the reduction in the funds rate.

Period 3, August 1982 through December 1983, is one of economic recovery. Initially, the funds rate, still high from the prior period of monetary tightness, falls sharply. It then is kept basically unchanged. In period 4, February 1984 through August 1984, the FOMC raises the funds rate even though predictions of GNP growth fall. This rise compensates for the moderation in the rise in the funds rate, relative

[1]Through 1987, the predictions in Figures 1 through 5 are from the table, "Gross National Product and Related Items," in the Board of Governors staff document, *Current Economic and Financial Conditions*, commonly referred to as the "Greenbook." (FOMC documents are released to the public after five calendar years.) Starting in 1988, the predictions are from the table, "Data Resources Forecast of the U.S. Economy-Control," in DRI/McGraw Hill, *Review of the U.S. Economy*. Through 1987, the funds rate is the value the desk expected to prevail at the beginning of the first full reserve settlement period following an FOMC meeting. It is taken from the New York Fed document, *Report of Open Market Operations and Money Market Conditions*. Thereafter, the funds rate is the daily average value for the first full week ending Wednesday following an FOMC meeting.

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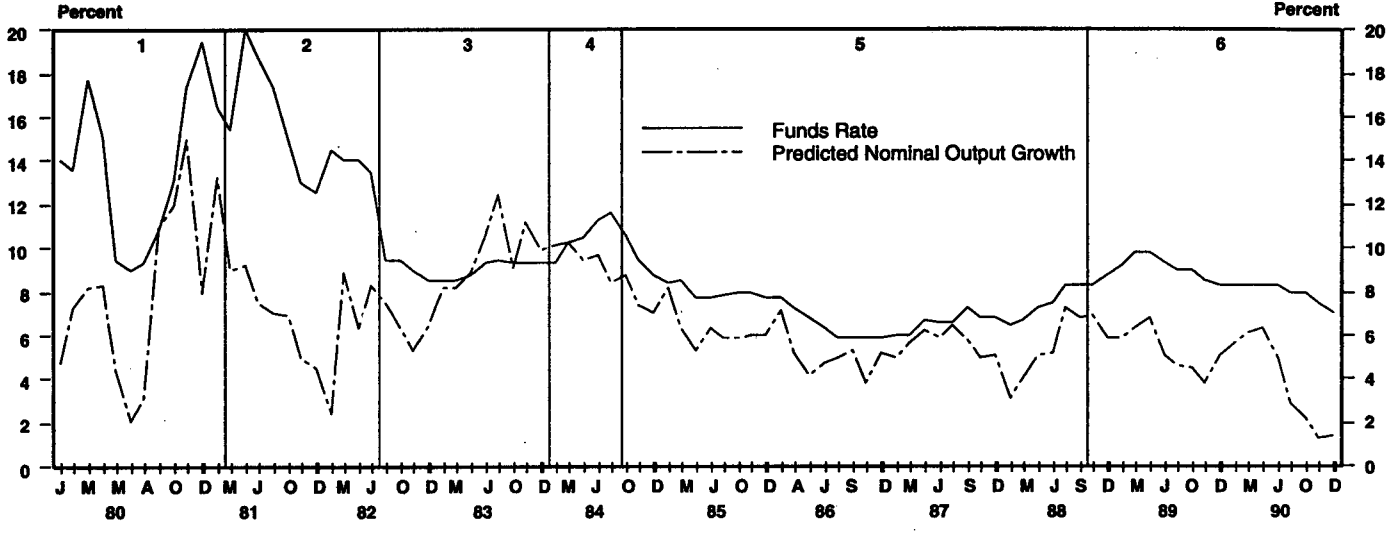
FIGURE 1

PREDICTED NOMINAL OUTPUT GROWTH AND FUNDS RATE



NOTES: Predictions available as of FOMC meetings of annualized quarterly rates of growth of nominal output. For FOMC meetings in the first or second month of a quarter, the prediction is for the contemporaneous quarter. For FOMC meetings in the last month of a quarter, the prediction is for the subsequent quarter. Predictions are from the Greenbook through 1987 and from DRI thereafter. The funds rate is the target set at FOMC meetings. It is from Report of Open Market Operations through 1987 and from market newsletters thereafter. Heavy tick mark indicates December meeting.

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to the rise in nominal GNP growth, in the earlier period. In period 5, October 1984 through September 1988, changes in the funds rate are primarily policy maintaining. They follow changes in nominal GNP growth fairly closely. In period 6, November 1988 through October 1990, the lag in the decline in the funds rate relative to the decline in predicted nominal GNP growth reflects policy-altering behavior. The thrust of monetary policy became restrictive toward the end of 1988.

Humphrey-Hawkins Testimony

Since July 1979, at the February and July FOMC meetings, members have submitted their predictions of growth rates for nominal GNP, real GNP, and inflation for the current year. The chairman makes these predictions public as part of his Humphrey-Hawkins testimony to Congress. They are presented in the form of a range that includes a smaller range called the "central tendency." The midpoint of this central-tendency range for nominal GNP growth can be a proxy for the FOMC's intermediate nominal GNP target. The midpoints of the central-tendency range for real GNP growth and inflation can be a proxy for the FOMC's objectives.

It is unlikely that individual FOMC members view their own predictions as objectives. For example, if the nominal GNP prediction were considered to be an intermediate target, individual FOMC members would choose a higher or lower value depending upon their predilection for a more expansionary or more restrictive monetary policy. Individual predictions, in contrast, are necessarily contingent on how the individual members believe the FOMC will behave over the year. For example, the "Record of Policy Actions" for the February 1990 meeting states, "In making these forecasts, the members took account of the Committee's policy of continuing restraint on demand to resist any increase in inflation pressures and to foster price stability over time" (Board *Annual Report* 1990, p. 93). The average of the predictions of FOMC members then can be considered as based on the monetary policy that a majority expect will be adopted. In this sense, these predictions are considered by the majority of FOMC members as acceptable outcomes and can be used as proxies for FOMC objectives.

In Figure 2, the solid line is the midpoint of the central tendency of FOMC members' predictions of nominal GNP growth presented in the chairman's most recent Humphrey-Hawkins testimony. The dashed line shows predictions available at the time of the FOMC meeting of the quarterly growth rate of nominal GNP for the quarter

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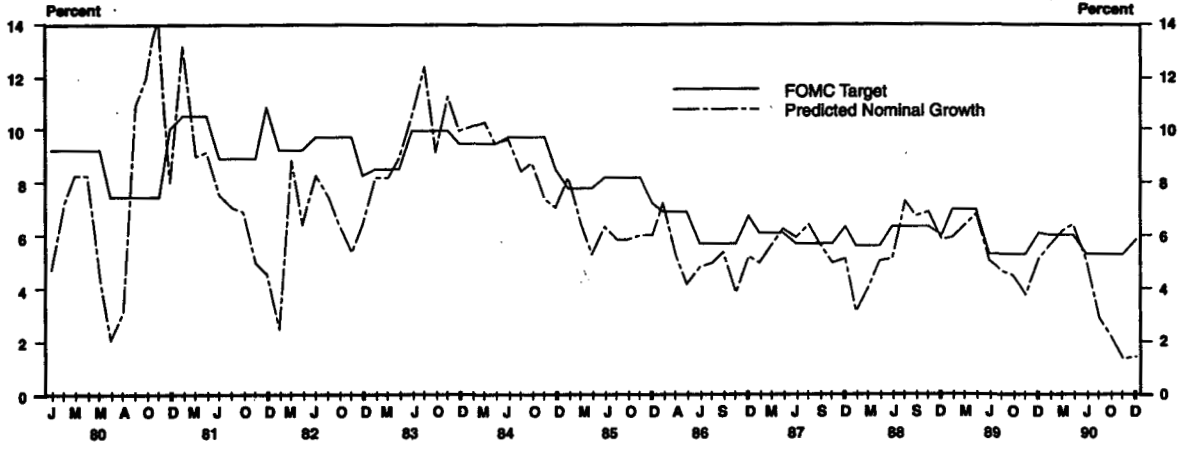
FIGURE 2

PREDICTED AND TARGETED QUARTERLY NOMINAL OUTPUT GROWTH



NOTES: Predicted nominal output growth is an estimate of the annualized quarterly rate of growth of nominal output available at FOMC meetings. If an FOMC meeting is in one of the first two months of a quarter, it is for the contemporaneous quarter. If the meeting is in the last month of a quarter, it is for the succeeding quarter. Predictions are from the Greenbook through 1987 and from DRI thereafter. Figures are drought-adjusted for the 8/88 to 3/89 meetings. Targeted nominal output growth is the midpoint of the central tendency range of fourth-quarter to fourth-quarter growth rates predicted by FOMC members and presented at the February or July Humphrey-Hawkins hearings. As of the July FOMC meeting, the target is inferred from the revised central tendency range for yearly growth and from estimated growth over the first two quarters. For December FOMC meetings, the target, which was presented at the previous July hearings, is for the subsequent year. Observations correspond to FOMC meetings and letters indicate month in which the FOMC meeting was held. Heavy tick mark indicates a December meeting.

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in which the FOMC meeting occurred.[2] Figure 3 shows the deviations between the predictions of current quarter nominal GNP growth and the central tendency figures plotted in Figure 2 (the dashed minus the solid line). These deviations are used as proxies for misses of nominal GNP growth from the FOMC's intermediate target for nominal GNP growth and are plotted along with changes in the funds rate. Changes in the funds rate that correlate positively with these misses are assumed to be policy-maintaining changes in the funds rate. Other behavior of the funds rate is assumed to be policy altering. For example, policy-altering changes in the funds rate occur from March 1981 through May 1982. Despite the shortfall in nominal GNP growth from its central-tendency value, the funds rate increases as well as decreases.

Money and Monetary Policy

During his Humphrey-Hawkins testimony, the chairman also announces target ranges for growth (fourth quarter to fourth quarter) of the monetary aggregates—M2, M3, and debt. These target ranges, however, lack operational significance. This fact is noted by standard references to the instability of money demand. For example, Chairman Greenspan (1989b, p. 7) stated:

> In view of the apparent variability, particularly over the short run, in the relationships between the monetary aggregates and the economy, policy will continue to be carried out with attention to a wide range of economic and financial indicators. The complex nature of the economy and the chance of false signals demand that we cast our net broadly—gathering information on prices, real activity, financial and foreign exchange markets, and related data.

Occasionally, when the FOMC has felt that money growth was reflecting the behavior of nominal GNP growth, it has used money as one of a number of informational variables for inferring the behavior of nominal GNP growth. In this way, the behavior of money could be taken account of without any need to modify the lean-against-the-wind procedures that use nominal GNP growth as an intermediate target for controlling real output growth and inflation.

In general, however, the FOMC considers M2 targeting to be incompatible with its lean-against-the-wind procedures, which require that the funds rate and economic activity move together. Consider a situation where economic activity strengthens. Market rates of interest rise, not only absolutely, but also relative to the

[2]For FOMC meetings from August 1988 through March 1989, predictions of nominal GNP (and real GNP in Figure 4) include a drought adjustment calculated by the Commerce Department and highlighted in the Greenbook.

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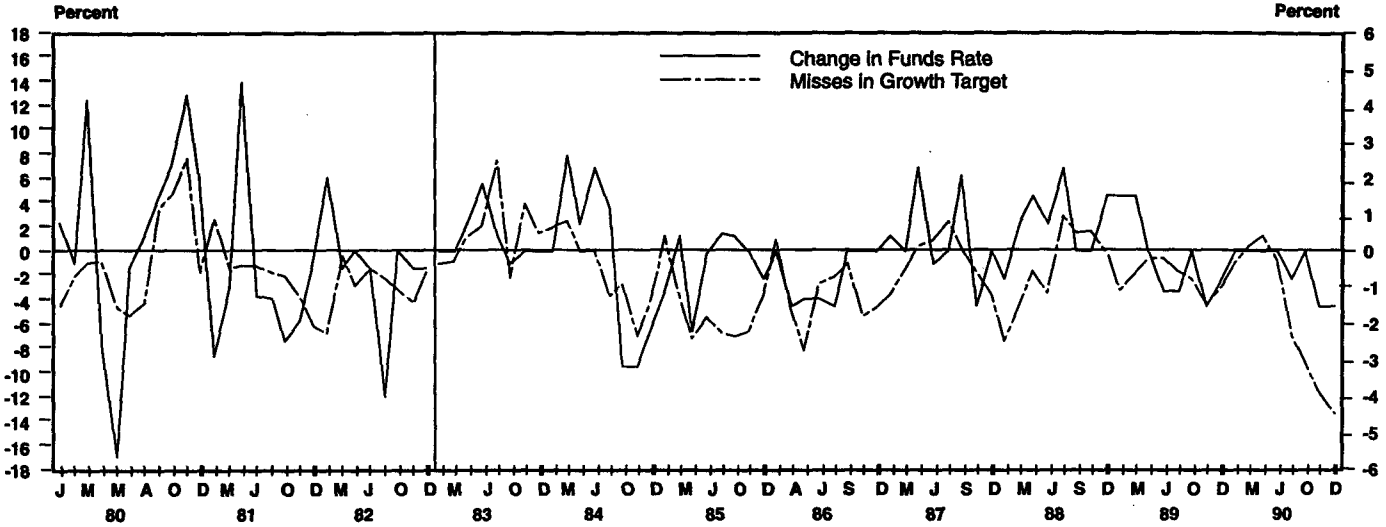
FIGURE 3

MISSES FROM TARGET OF QUARTERLY NOMINAL OUTPUT GROWTH AND FUNDS RATE CHANGES



NOTES: Misses from target are the differences between the predicted and targeted values shown in Figure 2. Left scale is for the 1980–1982 period. Right scale is for the subsequent period. The funds rate is the target set at FOMC meetings. It is from Report of Open Market Operations through 1987 and from market newsletters thereafter. Changes in the funds rate are multiplied by three. Heavy tick mark indicates December meeting.

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average of rates paid on the deposit liabilities in M2. Until the early 1980s, most of the interest-bearing components of M2 were subject to Regulation Q ceilings. Even after the elimination of Reg Q, banks have moved the interest rates paid on the components of M2, other than retail certificates of deposit, only sluggishly in response to changes in market rates. As a consequence, when market rates rise, the cost of holding M2 rises, individuals move out of M2 into other liabilities like large certificates of deposit, and the rate of growth of M2 falls. It follows that strength in economic activity is initially associated with a reduction in M2 growth and, conversely, weakness in economic activity is initially associated with an increase in M2 growth. M2 growth, therefore, does not consistently give the same signals about how to change the funds rate as nominal GNP growth.

Since the early 1900s, however, annual M2 velocity has persistently returned to a value of about 1.6. Although deviations from this value occur because of cyclical changes in interest rates that alter the opportunity cost of holding M2, these changes cancel out over time and leave M2 velocity unchanged. In principle, the FOMC could take advantage of the long-run stability of M2 velocity by eliminating base drift in its M2 target. It could still allow the rate of growth of M2 to vary inversely with the level of interest rates, but not allow M2 to drift randomly over long periods. Operationally significant, multiyear M2 targets, however, would conflict with the FOMC's desire for discretion. The FOMC has always felt that multiyear targets for money would constrain its freedom to set the funds rate discretionarily period by period.

FOMC Policy Actions: 1986–1990

Toward the end of the 1980s, the FOMC attempted a "soft-landing" strategy. It tried to lower inflation without a recession by keeping real growth moderately below its long-run trend. The strategy failed, in large part, because of the FOMC's reluctance to lower the funds rate significantly when economic activity began to weaken. This reluctance arose because of a fear that reductions in the funds rate would undermine the credibility of its resolve to reduce inflation. The financial markets remained skeptical of the FOMC's resolve because of the contradiction between its objective of lowering inflation and the persistence of relatively high inflation rates. The trend rate of inflation rose toward the end of the 1980s as a consequence of the FOMC's earlier attempt in the mid-1980s to maintain economic growth rather than to lower inflation. The follow-

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ing sections provide an overview of the FOMC's policy actions from 1986 through 1990.

1986: Expansionary Policy Actions

In 1986, the FOMC intended for monetary policy to produce a moderately stimulative impact on the economy. The midpoint of the FOMC's central tendency for real GNP growth, announced by the chairman in his February 1986 Humphrey-Hawkins testimony, was 3.25 percent. Chairman Volcker (Board *Annual Report* 1986, p. 36) testified: "The expanding job opportunities associated with the increase in output are expected to *lower* the unemployment rate gradually" (emphasis added). Throughout 1986, the FOMC believed that declining inflation allowed it room to lower the funds rate to stimulate the prevailing modest rate of growth of real GNP.

Figures 4 and 5, respectively, display the FOMC's central tendency for real GNP growth and inflation along with ongoing predictions of these variables. Starting in 1986, the predictions are from DRI, not the Board staff, but the various "Records of Policy Actions" for 1986 indicate that the predictions are in line with predictions available to the FOMC. The first sentence of the "Record" always summarizes the FOMC's consensus view of contemporaneous economic activity. For example, the "Record" for the February 12, 1986, meeting begins with the statement, "The information reviewed at this meeting suggested that economic activity was expanding at a moderate pace" (Board *Annual Report* 1986, p. 93). The tone of this statement summarized the FOMC's and Board staff's views about the economy throughout 1986 as reported in the "Record." The exception was the concern expressed by some members in the second quarter about weakness in energy-producing regions caused by the fall in the price of oil. This weakness, however, was viewed as transitory.

Figure 4 shows that ongoing predictions of real GNP growth were close to the FOMC's central tendency for both 1985 and 1986. In contrast, Figure 5 shows ongoing predictions of inflation lay well below the FOMC's central tendency for inflation for these years. Moderate real growth and falling inflation prompted the FOMC to lower the funds rate. In 1986, the funds rate fell from 8 to 6 percent.

The Board of Governors, as opposed to the full FOMC, took the lead in reducing the funds rate by reducing the discount rate. The Fed's operating procedures entail setting a target for the reserves banks borrow from the Fed. With these procedures, the funds rate equals the discount rate plus a positive amount that increases as

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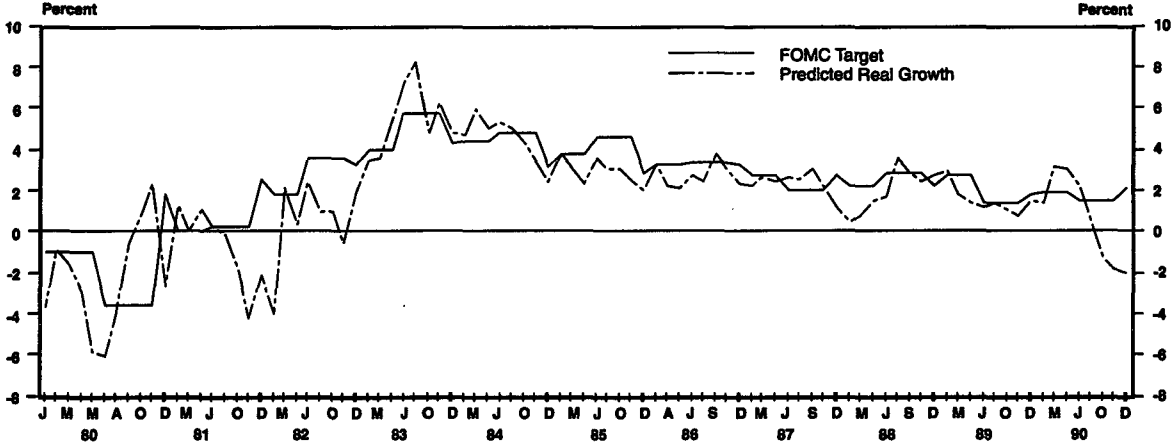
FIGURE 4

PREDICTED AND TARGETED QUARTERLY REAL OUTPUT GROWTH



NOTES: Predicted real output growth is an estimate of the annualized quarterly rate of growth of real output available at FOMC meetings. If an FOMC meeting is in one of the first two months of a quarter, it is for the contemporaneous quarter. If the meeting is in the last month of a quarter, it is for the succeeding quarter. Predictions are from the Greenbook through 1987 and from DRI thereafter. Figures are drought-adjusted for the 8/88 to 3/89 meetings. Targeted real output growth is the midpoint of the central tendency range of fourth-quarter to fourth-quarter growth rates predicted by FOMC members and presented at the February or July Humphrey-Hawkins hearings. As of the July FOMC meeting, the target is inferred from the revised central tendency range for yearly growth and from estimated growth over the first two quarters. For December FOMC meetings, the target, which was presented at the previous July hearings, is for the subsequent year. Observations correspond to FOMC meetings and letters indicate month in which meeting was held. Heavy tick mark indicates December meeting.

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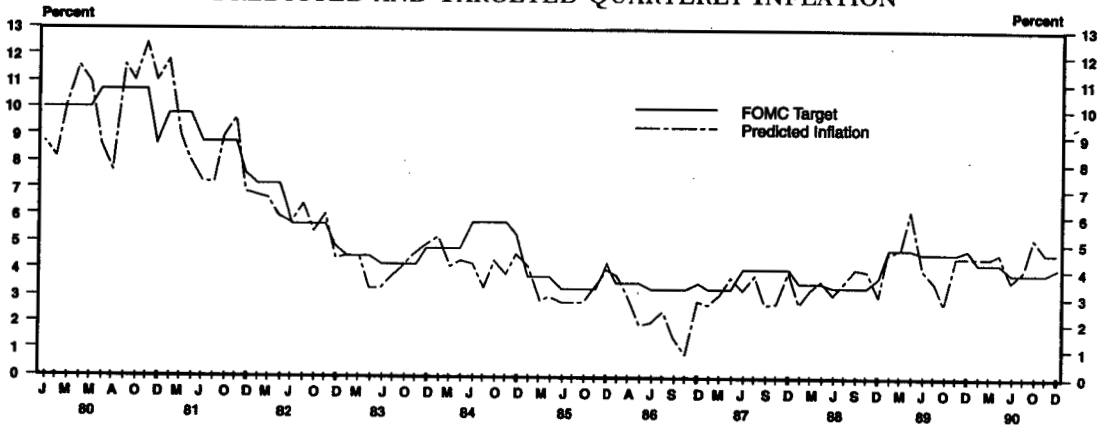
FIGURE 5

PREDICTED AND TARGETED QUARTERLY INFLATION



NOTES: Through 1988 predicted inflation is an estimate available at FOMC meetings of the annualized quarterly rate of growth of the implicit deflator for nominal GNP. From February 1989 through November 1989, predictions are for the CPI and, thereafter, for the CPI excluding food and energy. If an FOMC meeting is in one of the first two months of a quarter, the prediction is for the contemporaneous quarter. If the meeting is in the last month of a quarter, it is for the succeeding quarter. Predictions are from the Greenbook through 1987 and from DRI thereafter. Targeted inflation is the midpoint of the central tendency range of fourth-quarter to fourth-quarter inflation rates (GNP implicit price deflator through 1988 and CPI thereafter) predicted by FOMC members and presented at the February or the July Humphrey-Hawkins hearings. As of the July FOMC meeting, the target is inferred from the revised central tendency range for yearly inflation and from estimated inflation over the first two quarters. For December FOMC meetings, the target, which was presented at the previous July hearings, is for the subsequent year. Observations correspond to FOMC meetings and letters indicate month in which the FOMC meeting was held. Heavy tick mark indicates December meeting.

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the level of borrowed reserves increases.[3] A key feature of these procedures is that, for a given borrowed reserves target, a change in the discount rate initially changes the funds rate by the same amount. Under borrowed reserves targeting, the full FOMC sets the target for *borrowed reserves*. As evidenced by the "Records of Policy Actions," under Chairman Volcker, discussion at FOMC meetings was in fact limited to the choice of a borrowed reserves target. The Board of Governors, however, sets the *discount rate*. The Board, therefore, can move the funds rate independently of the actions of the FOMC. The "Records" for 1986 indicate that the entire FOMC, which includes regional bank presidents, had a cautious attitude toward reducing the funds rate. The Board, however, moved the funds rate down aggressively through discount rate reductions.

At the February 11 FOMC meeting, available statistics indicated that "economic activity was expanding at a moderate pace" (Board *Annual Report* 1986, p. 93). The "Record" states, "In these circumstances, nearly all participants agreed that little or no change in reserve availability was warranted" (Board *Annual Report* 1986, p. 100). Specifically, the FOMC voted for an unchanged borrowed reserves target and, by implication, an unchanged funds rate. It also adopted the following "symmetric" directive, that is, a directive with contingent language that did not predispose the chairman to move the funds rate in a particular direction in the intermeeting period (Board *Annual Report* 1986, p. 102):

> Somewhat greater reserve restraint or somewhat lesser reserve restraint *might* be acceptable depending on behavior of the aggregates, the strength of the business expansion, developments in foreign exchange markets, progress against inflation, and conditions in domestic and international credit markets [emphasis added].

The use of the word "might" rather than "would" is designed to render funds rate changes unlikely in the intermeeting period.

The decisions made at the February FOMC meeting indicated a majority sentiment in favor of no change in the funds rate. Although the contingent language of the directive is extremely vague, it proba-

[3]The desk sets a level of nonborrowed reserves less than the desired (required plus excess) reserves of banks so that the banking system must borrow the targeted amount from the discount window. At the same time, the Fed limits how often individual banks can borrow. Given their borrowing history, banks will borrow when they believe that current market rates are high relative to expected future market rates. Consequently, there exists a (noisy) positive relationship between borrowed reserves and the difference between the funds rate and the discount rate. An increased target for borrowed reserves increases the difference between the funds rate and the discount rate and, given the discount rate, increases the funds rate and other money market rates (Goodfriend 1983).

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bly indicated an emphasis on incoming data from the real sector. Incoming data indicated that real output was growing fairly strongly. Both inside and outside the Fed, the most widely watched statistic on the real sector is the monthly non-farm civilian payroll employment figure from the Labor Department. This statistic is the first available, comprehensive indicator of the behavior of real GNP. On February 7, the employment figure had registered a gain of 566,000, the largest gain in the decade of the 1980s. The average employment gain in the previous three months of 210,000 indicated steady growth in the economy. On March 6, however, the Board voted to reduce the discount rate by ½ percentage point. The reduction carried through to the funds rate, which fell from around 7⅞ to 7⅜.

Earlier, on February 24, 1986, the Board had voted 4 to 3 to reduce the funds rate by ½ percentage point to 7 percent. Opposition among Board members to the discount rate cut arose from a concern that a failure of foreign central banks to lower their discount rates in tandem would cause the dollar to depreciate (Board *Annual Report* 1986, p. 81). Newspapers reported that this split vote was the first time the Board had outvoted its chairman. The Board rescinded its action on the same day. It then voted unanimously to lower the funds rate on March 6. (The figure on payroll employment came out the day after, March 7. Presumably, the Board acted on March 6 to avoid the possibility of lowering the discount rate right after the release of a strong employment figure. In the event, the employment gain was a moderately strong 226,000.) Key foreign central banks did not lower their discount rates in 1986. Subsequent reductions by the Board in the highly visible discount rate, however, were apparently made to pressure them. The *Annual Report* (1986, p. 82) noted in connection with the July 10 reduction in the discount rate, "[A] reduction in the System's rate might encourage easing measures abroad later, if not immediately."

At the May meeting, the FOMC voted for an unchanged borrowed reserves (funds rate) target and for a symmetric directive. At its July and August meetings, the FOMC also adopted a symmetric directive, which signals no predispostion to change the funds rate in a particular direction. At the July and August meetings, respectively, it also voted to "decrease somewhat" and to "decrease slightly" the "existing degree of pressure on reserve positions." The use of the adjectives "somewhat" and "slightly" suggested a reduction in the funds rate of ⅛ to ¼ of a percentage point. After each of these three FOMC meetings, however, the Board lowered the discount rate and the funds rate by ½ percentage point. In 1986, with the Board taking the lead, the Fed concentrated on promoting real growth.

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More generally, in the mid-1980s, monetary policy did not attempt to lower inflation below its trend rate of about 4.5 percent, but concentrated on stimulating economic growth. As described below, beginning in the middle of 1988, monetary policy began to be directed toward lowering inflation. At the same time, the trend rate of inflation had begun to rise. This behavior of inflation made it difficult for the Fed to make its policy of reducing inflation credible. The bond markets in particular were sensitive to signs of increasing inflation. As a consequence, when economic activity weakened, the FOMC was reluctant to lower the funds rate. It feared that reductions in the funds rate would lead to increases in bond rates. The resulting rate smoothing produced the kind of sustained monetary deceleration that has preceded other recessions.

1987: Reacting to Financial Markets

FOMC actions in 1987 were dominated by a concern over the adverse behavior of the bond market and the stock market. Through early April, the funds rate was still only somewhat above 6 percent, essentially unchanged from late August 1986. Toward the end of March 1987, however, concern in financial markets over the continued depreciation of the dollar caused bond yields to rise. The FOMC responded to sharply rising rates in the bond markets in late March by moving the funds rate up to about 6.75 percent after its May 19 meeting. As Chairman Volcker stated later in his July Humphrey-Hawkins testimony, "[T]he Federal Reserve has given a great deal of weight to . . . episodes of heavy downward pressure on the dollar [and] indications from long-term securities and commodity markets of heightened inflationary expectations" (Board *Annual Report* 1987, p. 58). Again, in early September, in response to a rise in bond yields prompted by a falling dollar, the FOMC raised the funds rate, this time to somewhat above 7 percent.

The predictions in Figures 4 and 5 of real growth and inflation in 1987 agree with qualitative statements in the "Record." Through the FOMC meeting on September 22, 1987, the summaries of both Board staff and FOMC members generally contained phrases such as "economic activity has continued to expand at a moderate pace." After the stock market crash on October 19, 1987, the Board staff and FOMC lowered sharply their predictions for the rate of growth of real GNP. The "Report" for the November 3 FOMC meeting stated (Board *Annual Report* 1987, pp. 136–37):

> The staff projection suggested that the decline in equity prices would lead to weaker economic growth. . . . Members commented that the staff forecast of somewhat reduced economic growth over

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> the next several quarters was a reasonable expectation, but one that presumed the return of confidence and more normal conditions in financial markets. Accordingly, the risks of a different outcome, notably in the direction of more weakness, were viewed as much greater than usual.

Immediately after the crash, in a telephone conference, the FOMC lowered the funds rate to 6.75 percent from about 7.5 percent.

1988: Surprising Economic Strength

Instead of faltering after the October stock market crash, the economy continued to grow strongly. By spring 1988, the FOMC became concerned that strong growth would increase resource utilization enough to revive inflation. Figures 4 and 5 show, respectively, the FOMC's central tendency predictions for real GNP growth and inflation and DRI's predictions of the contemporaneous behavior of these variables. Figure 4 shows predicted real GNP growth strengthening and then remaining strong throughout 1988. This pattern of strengthening predictions accords with the qualitative descriptions in the "Record." The irregularly rising pattern of predicted inflation shown in Figure 5 explains much of the behavior of the FOMC from 1988 through fall 1990. Predicted inflation reached a trough in 1986 and then rose irregularly through 1990. Volatility in energy and food prices especially imparted volatility to inflation measures. This volatility in turn imparted volatility to financial markets.

Between the February 9 and March 29 FOMC meetings, the dominant concern among FOMC members changed from slow growth to rising inflation. On March 4, the payroll employment figure showed a rise in February of 531,000, with the January figure revised up to 174,000. Those figures came after a rise in employment of 3 million in 1987 and a reduction in the employment rate to 5.75 by year-end, the lowest level since 1979. The dominant theme of FOMC meetings in 1988 was first expressed in the March meeting. A majority believed that capacity utilization and the unemployment rate had fallen to the point where further declines would exacerbate inflation. At the same time, increasing demands were being placed on resources because of rapidly growing exports. By the end of 1987, relative to a trade-weighted average of the currencies of the other G-10 countries, the dollar had declined by almost 45 percent from its peak in February 1985. From its peak in 1986Q3 through 1988Q2, export growth caused the real merchandise trade deficit (1982 dollars) to decline by about $7 billion. A majority of FOMC members believed in 1988 that, in order to control inflation, interest rates would have

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to rise to depress domestic demand sufficiently to allow for increased foreign demand.

1989: The Soft-Landing Strategy

Sometime in 1988, the Fed put together the soft-landing strategy to lower the rate of inflation without a recession by keeping the rate of growth of real GNP somewhat below its long-run trend. This strategy can be summarized using the model of the first part of the paper. The FOMC tried to reduce inflation through a moderate reduction in the rate of growth of nominal GNP, its intermediate target. The reduction in nominal GNP growth would reduce the rate of inflation, the ultimate target, but only after an initial reduction in the rate of growth of real GNP. Real GNP, however, would be kept growing through the moderation in the reduction in nominal GNP growth.

The strategy was contained in Chairman Greenspan's February 1989 Humphrey-Hawkins testimony. The desired reduction in GNP growth can be seen in the central-tendency projections the chairman supplied in his Humphrey-Hawkins testimony ("Monetary Policy Report," Board *Annual Report* 1989, p. 29, Table 1). In order to see the reduction, however, we must adjust for the 1988 drought. The drought was assumed to have reduced the growth rate of nominal and real GNP in 1988 and then to have increased it in 1989 by a corresponding amount (around ⅔ of a percentage point). If the 1989 central tendency prediction is reduced to account for a rebound due to the reduction in output from the 1988 drought, the FOMC's central tendency predictions for nominal and real GNP growth were lower in 1989 than in 1988.

The following excerpt from the February 1989 Humphrey-Hawkins testimony by Chairman Greenspan (1989a, pp. 164, 167) describes the soft-landing strategy:

> Overall inflation, in the area of 4 to 4½ percent, during 1988 was a little above the general range in which it had fluctuated in the mid-1980s. . . . [L]et me stress that the current rate of inflation, let alone an increase, is not acceptable, and our policies are designed to reduce inflation in coming years. This restraint will involve containing pressures on our productive resources and, thus, some slowing in the underlying rate of growth of real GNP is likely in 1989. The central tendency of GNP forecasts for this year of Board members and Reserve Bank presidents is 2½ to 3 percent; abstracting from the expected rebound from last year's drought losses, real GNP is projected to grow at closer to a 2 percent rate.

The quotation implies that in 1989 the FOMC set a target for real GNP growth somewhat lower than the long-term trend rate. The

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assumption that the rate of inflation falls only after a sustained reduction in nominal GNP growth then implied an intermediate target for nominal GNP growth equal to this figure for below-trend real growth plus the prevailing rate of inflation. Specifically, the sum of the figures for inflation of 4.5 percent (approximately the 1988 rate) and for real growth of 2.5 percent implied a target for growth in nominal GNP of 7 percent for 1989. The latter figure is the midpoint of the FOMC's central tendency projection of GNP growth.

The desire by the Fed to lower the rate of inflation led to policy-altering behavior of the funds rate in early 1989. When the real sector strengthened in spring 1988, the FOMC began to raise the funds rate. The considerable stability of bond rates in 1988 suggested that the rise in the funds rate was appropriate for restraining increased aggregate demand. The strength in the real sector continued until early 1989, but then began to moderate when improvement in the trade deficit stalled. Because of its decision to reduce the inflation rate by targeting a low rate of growth of real output, when real output growth weakened in 1989, the FOMC maintained the funds rate at 9.75 percent, rather than lowering it. Incoming statistics on the economy continued weak, and the bond market signaled clearly that the thrust of policy was restrictive. The 30-year bond rate peaked on May 10 at 9.1 percent. With no change in the funds rate, it then began to fall sharply until, by the end of June, it was at 8 percent. Beginning in early June, the FOMC began to work the funds rate down through quarter-point reductions.

Declines in bond yields in the last half of 1989 indicated that the markets believed that the moderate decreases in the funds rate in 1989 were appropriate to offset weakness in the rate of growth of aggregate demand. A decrease in the funds rate in the beginning of January 1990, however, was followed by a rise in bond rates. Concern in the bond market over inflation limited the FOMC's willingness to lower the funds rate further until fall 1990.

1990: Recession

Inflation rose in 1990Q1. In part, the rise was due to severe winter weather that forced up food and energy prices. The trend rate of inflation, however, appeared to be moving up from 4.5 to almost 6 percent. A rise in broad measures of labor compensation reinforced concern about the behavior of the trend rate of inflation. The July 18 "Monetary Policy Report to the Congress" (Board *Annual Report* 1990, p. 51) stated:

> In the early part of 1990, economic activity appeared to be regaining momentum, a development that reduced previous concerns about

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> recessionary risks. At the same time, even discounting weather-related spurts in food and energy prices . . . there appeared to be no abatement in underlying inflationary pressures. . . . [I]n keeping with the tenor of most of the economic data released during the quarter, other interest rates generally moved higher, particularly at the long end of the yield curve.

At its March 27 meeting, the FOMC was still concentrating on inflation (Board *Annual Report* 1990, pp. 104, 105):

> The members viewed sustained growth in business activity as a reasonable expectation for the next several quarters. . . . [T]he prospects for inflation remained the most disturbing aspect of the economic outlook.

In the uncertainty surrounding the invasion of Kuwait on August 2, the FOMC kept the funds rate pegged at around 8 percent. The August 21 "Record" stated (Board *Annual Report* 1990, p. 133):

> With the surge in oil prices tending to weaken economic activity while also intensifying inflationary pressures, an easing in policy would incur the risk of overcompensating for potential weakness in the economy at the expense of greater inflation, while a tightening move to counter inflation might stall an already weak economic expansion.

The FOMC was also concerned that a reduction in the funds rate would produce a rise in bond rates. Chairman Greenspan (1990, p. 930) testified on September 19 before the Joint Economic Committee:

> Another key issue one must address is how much of any change in short-term rates would carry over to the crucially important long-term rates, given the concern in financial markets about prospects for inflation. . . . Policy actions that are not perceived to be consistent with a stable, noninflationary economic environment could easily become counterproductive over the long haul.

A business cycle peak occurred in July 1990, before the invasion of Iraq. The increased price of oil produced by the invasion must have exacerbated the recession directly. It exacerbated the recession indirectly through the heightened concern it produced on the FOMC over inflation. That concern caused the FOMC to postpone the process of working the funds rate down in response to weakness in the economy. Nevertheless, the cycle peak occurred before the invasion. Payroll employment (survey data of strike-adjusted, non-farm, non-census workers) grew at an annualized rate of 2.5 percent in the first quarter of 1990, but virtually ceased growing in the second quarter, and fell in July. Aggregate hours worked (payroll employment survey) behaved similarly. After the release on December 7, 1990, of

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figures showing a decline in payroll employment of 265,000 for November and a revised decline of 180,000 for October, the FOMC began to move the funds rate down sharply.

Evaluating the Soft-Landing Strategy

One can argue that the Fed's soft-landing strategy failed because of bad luck. In the winter of 1989–90, poor weather augmented inflation by pushing up food and energy prices. Overbuilding in commercial real estate and weakness in bank capital caused a general tightening of credit standards. Finally, the sharp rise in oil prices after the invasion of Iraq undoubtedly depressed real output. On the other hand, one can argue that any strategy must account for bad luck. From this perspective, the soft-landing strategy failed because of the difficulty of setting the Fed's instrument, the funds rate, to achieve a desired objective for real growth.

In any event, the monetary policy actions preceding the 1990 recession resemble those preceding past recessions. The rate of growth of real GNP peaked in 1987Q4. The funds rate, however, did not peak until 1989Q2 and then declined only slowly thereafter. The FOMC felt constrained in lowering the funds rate because of the behavior of the bond market. Bond yields declined very little between 1987Q4 and 1990Q3. After 1989Q4, the moderate decline in the funds rate was accompanied by a rise in bond yields. The bond markets remained preoccupied by inflation.

The Fed kept the funds rate up through monetary deceleration. In the mid-1980s, the trend rate of growth of M2 had been around 9 percent (apart from the high growth in 1983 associated with the introduction of money market deposit accounts). The trend rate of growth of M2 began to decline in 1987, but the depressing impact on nominal GNP growth was initially offset by a rise in the opportunity cost of holding real M2. Eventually, a decline in GNP growth followed the decline in M2 growth.

In an important respect, the experience of monetary policy in the 1980s has been encouraging. For much of the 1980s, changes in the funds rate were policy maintaining, rather than policy altering. The FOMC became more willing to alter the funds rate in response to changes in the rate of growth of real GNP, regardless of the phase of the business cycle. An increased willingness to make policy-maintaining changes in the funds rate lessened the inertia in funds-rate changes that produced the stop-go monetary policy of the 1960s and 1970s. The relative stability over most of the 1980s of CPI inflation (excluding food and energy) suggests that monetary policy has

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become less of a source of macroeconomic disturbances. However, the Fed's recent strategy of working the inflation rate down without a recession failed. It appears that the Fed can pursue a monetary policy that maintains the rate of growth of nominal expenditure at a fairly stable value. The Fed encounters difficulty, however, when it attempts to alter the rate of growth of nominal expenditure.

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Asking the Right Question about Monetary Policy

Paul W. Boltz

Robert Hetzel presents an excellent analysis of what the Federal Reserve thought it was doing and why, and he provides a balanced review of what actually happened. I was particularly fond of the distinction he used to divide "policy maintaining" changes in the funds rate from "policy altering" changes. This distinction is akin, but not identical, to the Wall Street description of the Fed as "following rates" or "leading them." In this framework, the Fed's performance is worthy of the careful and well-nuanced analysis it receives. And while I might quibble here or there with Hetzel's review of the 1980s, the overall thrust of his paper strikes me as right on the mark.

The Volcker Standard

What the press came to call the "Volcker standard" worked pretty well, and Hetzel explains in detail the hows and whys. The problem is that the Volcker standard, or whatever you choose to call it, delivered lower inflation the old-fashioned way—with brutal recessions. When prosperity returned, inflation accelerated through the 1980s, except for 1986 when OPEC collapsed. To be sure, inflation did not move up as quickly as many had expected or get as bad, but 5.9 percent inflation in the first half of 1990, before Iraq's invasion of Kuwait, was not my idea of good news on the inflation front.

The Goal of Zero Inflation

The Federal Reserve has given considerable lip service to the goal of reducing inflation to zero or thereabouts in five years, as Rep.

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Stephen L. Neal (D., N.C.) proposed it should. However, the Neal proposal looks to me like the monetary policy equivalent of Gramm-Rudman-Hollings. So I would suggest that Hetzel explore precisely what the Fed should do differently in the future to keep inflation from creeping up. Alternatively, could the Fed have done something differently during the 1980s, short of recession, to slow inflation? The answer may be that the unemployment rate was pushed below the natural rate.

No Simple Solution for Monetary Policy

I do not know the answers, but I do not think they lie in any one approach to monetary policy. I tremble to think what would have happened if the FOMC had listened to monetarists in the 1980s, particularly in 1983 when Milton Friedman, writing in the The *Wall Street Journal*, predicted with characteristic gusto an imminent "surge" in inflation. The FOMC, the Fed staff, and market people like me have not forgotten those days. So I think it is important to ask the right question about monetary policy to get the right answer. I do not think the key question is "What single nominal variable should the Fed pay attention to?" As an epistemological matter, I do not know if there is any answer to that question, but it would surprise me if this very complicated world were so accommodating as to be simple in the area of monetary policy.

The Key Question

The question I would prefer—and Hetzel is well prepared to try to answer—is this: "What can the FOMC do within its institutional framework to make better monetary policy?" Precious little work has been done on this topic, especially by academic economists. As Eduard Bomhoff (1992) has pointed out, academics prefer highly abstract, often obtuse, formulations. My preferred question would recognize explicitly that the FOMC should look at many things to make its decisions. Hetzel's paper is a fine contribution to a start on answering the right question.

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Political Guidance on Monetary Policy

William A. Niskanen

This paper focuses on a narrow but important topic: the nature of political guidance to the Federal Reserve on monetary policy. The Constitution authorizes Congress "to coin Money [and] regulate the Value thereof." As a rule, however, Congress has delegated this authority to the Federal Reserve, either without guidance or, more recently, with sufficiently confused, redundant, or contradictory guidance to permit the Fed to chart its own course. We could do worse. The performance of the Federal Reserve has usually been better than that of most other central banks.

I believe we can also do better—much better—and will summarize a proposal for a new monetary rule along with a process for approving, implementing, and monitoring this rule.

Selecting a Monetary Policy Rule

First, I shall focus on the choice of a monetary rule. There are only three viable monetary rules. One is to maintain a path of the price of some specific commodity such as gold or some broader price index. Second is to maintain a path of some monetary aggregate such as the monetary base or M2. And third is to maintain a path of some measure of total demand in the economy such as nominal GNP or domestic final sales.

Any one of these rules would be better than guidance based on interest rates or exchange rates, or on any real variable such as the growth of output or the level of the unemployment rate. Some comments are in order, however, concerning the reasons for choosing one or the other of these three rules.

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A Price Rule

Wayne Angell and others have performed a valuable service by reviving the case for a price rule that is based on gold or some broader commodity index. The primary problem with a price rule is that the price of commodities depends on both demand and supply conditions, and a price rule can lead to considerable instability in other markets. The long experience with the several types of gold standards, for example, included several short periods of inflation caused by major gold discoveries, long periods of deflation, frequent recessions, and the Great Depression. The primary value of the gold standard was to prevent cumulative inflation over a much longer period of time. As best we can measure, for example, the U.S. price level in 1939 was about the same as in 1789. A price rule based on a broader set of commodities, however, would have the same general types of problems, although the variability of conditions in other markets would probably be smaller than one based on a commodity such as gold. In the modern world, for example, a price rule would require the Fed to deflate the general economy in response to an oil shock. I believe that we can do much better than that.

A Monetary Aggregate Rule

For about 20 years, most monetary economists promoted a rule to stabilize the path of some monetary aggregate such as M2. The case for this rule was based on a belief that there was a roughly stable relation between the level of the money supply, however measured, and the level of some measure of total demand in the economy. In other words, the objective of the monetarists was to stabilize the path of total demand, and they believed that a stable path of some measure of the money supply would best serve this objective. In response to this advice, the Fed has set targets for several monetary aggregates since 1970, and Congress has reviewed and approved these targets since 1975.

For most of the postwar years through 1981, the relation between total demand and money appeared to be roughly stable. There was a reasonable case that the increasing inflation and occasional recessions were primarily due to the Fed's failure—with rare exception—to stay within the approved target ranges for the money supply.

Since the end of 1981, however, the relation between total demand in the United States and each of the monetary aggregates has changed sharply. Specifically, total demand has continuously declined relative to the level of the money supply. The reasons for this continued decline in the velocity of money are not too clear, but it was probably due to the effect of the decline in market interest rates, the increase

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in interest rates on bank deposits permitted by deregulation, and, I think importantly, the substantial increase in the value of other financial assets.

Moreover, the accumulating econometric evidence indicates that the relationship between *changes* in total demand and *changes* in the money supply is roughly stable, but the relationship between the *levels* of these conditions is not stable. In technical terms, this relationship appears to be difference-stable but not trend-stable.[1] The most important implication of these findings is that a stable growth of the money supply will not lead to a stable growth of total demand. For this reason, the primary policy rule promoted by what I call the "high church monetarists" does not appear to be the best rule.

A Target Path of Total Demand

Another alternative is for Congress to approve a target path of total demand in the American economy.[2] This is best measured, I suggest, by what the Department of Commerce defines as "final sales to domestic purchasers." It would also be valuable to exclude purchases by the Federal Commodity Credit Corporation from this aggregate. These purchases, although they are measured as final sales, are actually increases in government inventories of farm products and are unusually volatile. The primary reason for selecting this variable rather than nominal GNP is that the demand for money in the United States appears more closely related to total purchases by Americans than to the dollar level of total output by Americans.

It is important to recognize that a demand rule is consistent with any desired price-level path, including a stable price level. (For this paper, I will avoid the issues that bear on the choice of the desired price-level path.) My primary point is that a demand rule is superior to a price rule, whatever is the desired price-level path, because of the different response to changes in supply conditions. A central bank following a demand rule would not respond to either positive or negative supply shocks; such shocks would lead to a one-time change in the combination of price and output changes in that year but would not lead to a long-term change in the inflation rate. A central bank following a price rule, however, would increase the monetary base in response to a positive supply shock and would tighten the base in response to a negative shock, thereby increasing

[1]This characteristic of the velocity of money was first identified by Gould and Nelson (1974), who used annual data, and confirmed by Haraf (1986), who used quarterly data.

[2]The case for a demand rule was first made in the early 1980s by Hall (1981), McCallum (1984), and Gordon (1985).

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the variance of output. Both rules are consistent with any desired price-level path. The primary case for a demand rule is that it reduces the variance of output.

In summary, a demand rule is superior to a price rule because it does not lead to adverse monetary policy in response to unexpected—either favorable or unfavorable—changes in supply conditions. Similarly, a demand rule is superior to a money rule because it accommodates unexpected changes in the demand for money. For these reasons, I suggest that implementation of a demand rule is the most appropriate next step for U.S. monetary policy.

Establishing and Monitoring a Demand Rule

How might a demand rule be approved, implemented, and monitored? The beginning of this process would be much like the current process. In February of each year, the administration would propose a target path of nominal domestic final sales for a several-year period as part of its projections in the budget and in the economic report. This proposed target path would reflect the combined effect of the administration's forecast of real final sales plus a recommended price-level path, reflecting how fast the administration proposes to reduce inflation. The Joint Economic Committee (JEC) would then review the proposed target path, in addition to those targets that may be proposed by the Fed, the Congressional Budget Office, and private economists. There need be no review or approval of the target ranges for any of the monetary aggregates, as is currently the case. This procedure would focus the attention of Congress on a value issue (the desired price path), not on the technical issues affecting the choice of the path of some monetary aggregate.

At this stage some change in procedure may be desirable. My preference would be to have the JEC approve a bill that would formally instruct the Fed to follow a specific target path of nominal domestic final sales, a bill that would then have to be approved by both houses of Congress and the president. Some formal legislative authority by the JEC, I believe, would also contribute to reviving this once important committee. My only reservation about this process is that Congress, as a body, has usually had an inflationary bias. But the central role of the JEC and a potential presidential veto, I think, probably should be enough to discipline this bias.

Once the target path for final sales is selected, the Fed would set an instrumental target for the monetary base, which is the sum of currency plus bank reserves, to implement the approved target path of nominal domestic final sales. This instrumental target would be

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selected based on the historical relation between the change in nominal final sales, which is what Congress has approved, and the change in the monetary base, which is what the Fed can control.

At this stage, there may be some opportunity to use current data from forward-looking auction markets, such as the several first suggested by Manuel Johnson (1988), to determine whether there is a likely change in the relation between changes in final demand and the monetary base. I am intrigued by this suggestion, but I am not yet convinced that one can improve on the type of adaptive rule suggested by Bennett McCallum (1984) that is based on the historical data on demand and the base, even though both series are subject to frequent revision. In any case, it is important to use data on commodity prices, interest rates, and exchange rates only as clues to setting the base target, rather than the current and quite dangerous practice of using the federal funds rate as an instrumental target.

There is no reason for an external review of this instrumental target for the monetary base. Most importantly, as often as once a quarter, the Fed would compare the actual final sales in the previous quarter with the approved target and would then change the target for the monetary base for the current quarter in order to return to the approved target path. This process, over time, should be designed to minimize the variance of actual final sales relative to the approved target, even though the process may increase the variance of changes in the monetary base. Over time, the path of nominal GNP will roughly track the path of nominal domestic final sales but with a somewhat higher variance due to changes in inventory accumulation and exports that are less affected by U.S. monetary policy. This process would not lead to a stability of all macro conditions, but minimizing the variance around an approved target path of nominal domestic final sales is probably the most that can be expected of monetary policy.

The third step in this process would be for the administration and Congress to monitor the Fed's performance, maybe as often as once a quarter. This review should focus on the reasons why actual final sales may have differed from the target path in the previous quarter. An increasing difference between the actual and the target final sales over a period as long as two quarters should automatically trigger such a review. There is ample reason to criticize the Fed for an accumulating difference between the actual final sales path and the approved target path. But as long as the Fed maintains a roughly stable level of final sales relative to this path, both the administration and Congress should refrain from criticizing the Fed because of a concern about a wide range of other conditions.

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Conclusion

In summary, we now expect the Fed to do too much. As a consequence, the Fed does not perform its most important function very well. At various times, our political system pressures the Fed to sustain the recovery, to reduce inflation, to reduce interest rates, to reduce the unemployment rate, to strengthen or weaken the dollar, to finance the government debt, or whatever.

One of the most important lessons of political economy is that a government must have at least as many policy instruments as it has goals. The Fed has only one policy instrument, specifically the level of the monetary base. It is important to focus this instrument on the single, most-important, achievable goal of monetary policy. That goal, I suggest, is a stable path of nominal domestic final sales. At the same time, it is important to recognize that we need to put our fiscal and regulatory house in order if we are to achieve our other economic goals.

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BOOK REVIEWS

Capital Markets and Development
Steve H. Hanke and Alan A. Walters, eds.
San Francisco: ICS Press, 1991, 385 pp.

Capital Markets and Development provides an excellent survey of theories and evidence for anyone who wants to be brought up to date on the movement toward more liberal financial markets now sweeping the developing countries. All of the authors of the papers and comments collected in this volume have had firsthand experience as advisers or officials, helping to roll back the thicket of financial controls and regulations that has stunted capital markets and development in much of Latin America and some Pacific Rim countries. The hard-won wisdom it contains could also be applied in the former Soviet Union and Eastern Europe.

In explaining why financial regulation and intervention became so omnipresent in developing countries, Steve Hanke and Alan Walters point out that many developing countries had been colonies of European powers. Their monetary and financial systems were linked to the financial systems of metropolitan centers in Europe. Currency boards limited monetary expansion in some of them. Inflation was modest; saving and investment were high; and a liberal economic order prevailed (whatever the political orders might have been). When colonies became independent states in the 1950s and 1960s, however, institutions identified as colonial were condemned. The new nations discovered central banks, which became instruments for generating seigniorage, financing deficits, and enabling officials to make capital allocation decisions.

The developing countries were told by economic theorists that they faced a "vicious circle of poverty," because their incomes and savings were too low to generate the capital necessary for development. To break the circle, with the help of foreign aid, governments should intervene, regulate, redistribute incomes, allocate credit, manipulate exchange rates and interest rates, and build public works projects to create the conditions for economic progress. Officials and their local and visiting economic advisers assumed that governments of developing countries must use various forms of forced saving, such as taxes and high reserve requirements on banks. The rulers of countries that had become independent much earlier, as in Latin America, also learned quickly how to use direct controls in their economies and financial systems to garner and

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deploy political power. One result: some formerly rich countries such as Argentina became poor.

In a retrospective assessment of Ronald McKinnon's path-breaking *Money and Capital in Economic Development* (1973), a book which provided inspiration for much of the more recent research reviewed here, Lawrence White extends the Hanke-Walters argument that rent-seeking politicians exploited the financial controls and the money-printing presses of developing countries for their own benefit. McKinnon expresses a more charitable view of politicians in the present book when he argues that the motivation of the state for intervening was relatively benign. When the public believes domestic capital markets are malfunctioning, he writes (p. 105), "[T]remendous pressure is placed on government to get in there and replace the moribund capital market with some other incentives." This argument becomes important when assessing the prospects for liberalization. The resistance of entrenched rent seekers can be formidable. Therefore, research on the role of financial institutions and capital markets, such as the work reported in Hanke and Walters's volume, is essential for winning popular support for financial liberalization.

McKinnon's analysis of politicians' motivations for intervening in financial markets was not his major contribution to our understanding of capital markets and economic development. What he and his Stanford colleague, Edward S. Shaw (1973), accomplished was to demonstrate that private financial intermediaries and free capital markets are crucial to economic development. In short, they said, financial intermediaries and capital markets are more efficient in facilitating domestic saving and allocating scarce capital resources (including foreign capital) than any system of governmental regulations and financial controls.

In what McKinnon calls "repressed markets," direct controls and over-regulation inhibit evolution of the very institutions and financial services that developing countries need most for gathering and allocating capital. Costs of regulation and rate ceilings of various kinds cause banks and other conventional financial institutions to lose market share to informal "curb" markets that may in turn become targets for regulators.

The crucial role of financial intermediaries and capital markets in economic development may seem too obvious to be worth discussing now. But it was not obvious to many of the people advising or ruling developing-country governments at the time McKinnon and Shaw wrote their books. McKinnon says he came to the importance of finance as a revelation from his own experience as a tariff adviser in South Korea during the 1960s, not from reading economic history. "The key role of financial institutions in the late 19th century for European and American development was neglected in the postwar literature emphasizing economic theory," he writes in his reply in the present volume to White's review (p. 105).

The rich array of evidence this book presents on damage done by financial controls should provide governments with incentive for

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continuing the current liberalization movement, if it is fully understood. However, there is another powerful incentive for financial liberalization. Governments are finding it more and more difficult to isolate their systems from financial markets in the rest of the world. Murray Sherwin, of the Reserve Bank of New Zealand, commented (p. 182), "The technological advances of funds transfers have really run way beyond any form of exchange control mechanism that I am familiar with at this stage. It is very easy to evade controls." Governments can no longer hold capital prisoner.

In some countries, the alleged "inadequacy of domestic financial markets" is still used to justify governmental interventions or as a reason for going slowly on liberalization. However, these arguments are losing force in the face of international competition. Capital flights can frustrate the efforts of the most zealous controllers. In effect, all governments today are challenged to allow their people and businesses free access to world capital markets if they want to develop their potential for economic development fully.

This book discusses advantages and disadvantages of various prescriptions for financial reform, fiscal and monetary policies, and the development of new institutions such as stock exchanges—too many to review here. Some of the most compelling arguments for reform come from Rolf J. Lüders, who was minister of finance and minister of economics in Chile during the 1982–83 crisis, and from Murray Sherwin, from the Reserve Bank of New Zealand, who played a leading role in the financial liberalization of New Zealand. From the Lüders account of liberalization in Argentina and Chile, one can get a sense of the sometimes maddening surprises that lurk in the path of reform programs. Nobody said it should be easy, and Lüders bears that out very well. Nevertheless, he stresses the need for sticking with the program, come what may. Argentina, he says, made the mistake of vacillating at critical moments, and so lost credibility in financial markets. Achieving credibility in liberalizing financial markets, as in fighting inflation, reduces adjustment costs.

Murray Sherwin comes to the same conclusion on the importance of credibility. He also makes some interesting comments on the difficult question of "sequencing" in reform programs, when to do what. Some economists, including McKinnon in this volume, argue that fiscal and monetary policies, and perhaps other policies, should be under firm control before it would be prudent to liberate exchange markets and commercial banks, for example. While in no way denying the need for sound fiscal and monetary policies, Sherwin says (p. 182), "My conclusion with respect to New Zealand is that the sequence of reforms was pretty much irrelevant. Speed and comprehensiveness were more important than the exact sequencing." He also makes an encouraging observation (p. 232), "The liberalization process, like the regulatory process that preceded it, feeds on itself. Just as regulations beget further regulations to close the inevitable loopholes and escape routes, the removal

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of regulations puts pressure on those remaining." That should be posted on the wall of every ministry of finance and central bank.

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San Francisco: Pacific Research Institute, 1990,
397 pp.

In the *Enterprise of Law*, Bruce Benson provides us with the most comprehensive treatise on private sector alternatives to government law enforcement available today. Benson systematically addresses all the issues, arguments, and objections surrounding the growing role of market institutions in the legal system. But his book is more than a mere defense of current privatization trends in protective services, corrections, and dispute resolution. The *Enterprise of Law* questions the seemingly axiomatic proposition that law and order are "necessary functions of government." Benson is fully aware that neither the policymaking community nor the general public is prepared to debate the *complete* privatization of all legal institutions. But the time is right, he argues, "to question the *presumption* that law and order must be governmentally provided." To Benson's credit, many readers will begin questioning that presumption even before completing the book.

The book begins with an examination of historical precedents of private, customary legal systems. Benson points out that many legal systems have developed "from below" as custom—as opposed to being imposed "from above," such as by a king or a legislature. This brief historical review, while sometimes tedious, is nevertheless a good starting point because the reader is forced to think about the basic premises of the American legal order.

Benson argues that customary legal systems are likely to include "an emphasis on individual rights because [in such a system] recognition of legal duty requires [the] voluntary cooperation of individuals through reciprocal arrangements." This is a persuasive argument, but the key word is "likely." One of Benson's own examples amply illustrates the exception.

Under the customary law of the Kapauku Papuans of West New Guinea, adulterous women were routinely executed by their husbands.

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Benson seems to play down this and countless other practices antithetical to individual rights that are commonly found in customary legal systems.

This is not to say that pernicious practices have not existed under governmental regimes. Benson's argument, however, suggests that customary law can do a better job of protecting individual rights than governmental institutions, including, presumably, limited constitutional institutions such as the tripartite form of government in the United States. That contention is less convincingly demonstrated than Benson's much stronger argument that the enterprise of law would be operated more *efficiently* by purely private institutions.

Indeed, the best part of the book is Benson's public choice analysis of the police and the courts. He shows how both institutions currently operate under perverse incentives that significantly reduce their overall effectiveness in providing quality service to customers. For example, public police forces often respond to criticism by touting statistics about an "increased number of arrests." Because such statistics are accepted as a measure of effectiveness in combating crime, police departments have the incentive to inflate the numbers by concentrating their resources on easy arrests and easily solvable crimes like prostitution and drug dealing. Violent crimes consequently receive less attention even though they are of the utmost concern to the taxpaying public.

Benson has many more insights along these lines, and some might make good reform-minded proposals. For example, why not hold individual citizens financially responsible for false alarms? Benson shows that false alarms are extremely costly as valuable police resources could be responding to important calls elsewhere.

Public choice analysis is also used to defend privatization efforts currently under way. Benson addresses all of the issues and standard objections with straightforward and persuasive arguments. Indeed, if the issue is whether privatization trends should continue in legal services, Benson makes a compelling case that they should.

Benson's more controversial thesis—that a private system of law and order can protect individual rights even better than a fundamentally reformed system of law here in the United States —is less compelling. He minimizes the enforcement problems associated with private legal systems. In fact, he sees little difference between public and private law enforcement. According to Benson, "government enforcement of property rights is based on the threat of violence in the same way that a customary property rights system is. . . . If someone refuses to comply with the government's rules, he is declared an outlaw." That may be true, but it is a rare situation when the lawbreaker is caught "red-handed." Investigations are often necessary to determine the *identity* of the offender. This is where the problem begins. How can an effective investigation proceed under a private process if the investigators have to rely on the voluntary cooperation of every witness and suspect? Benson notes that customary law is recognized "because each individual recognizes

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the benefits of behaving in accordance with other individuals' expectations" —"*not* because it is backed by the power of some strong institution." He suggests, without much discussion, that the incentives of reciprocity can protect individual rights better than any governmental system. This line of argument should have been developed in more detail, for as it stands, Benson's conclusion is hardly compelling.

Under the U.S. Constitution, the government may conduct a "reasonable search" if an independent magistrate agrees that the police have "probable cause" to believe that a particular citizen has committed a particular crime. U.S. citizens cannot stop or delay a search by challenging the government's finding of probable cause. A private police investigation, by contrast, could be thwarted by lack of consent, giving guilty persons the opportunity to destroy evidence of the crime and their participation in it. A woman, for example, could truthfully report that she had been raped during a party at a millionaire's compound. Under customary law, her private investigators might be met at the perimeter of the compound by the millionaire's own police, who explain that they have no intention of allowing a search of the premises while the festivities are still going on, but that they might allow a search the next evening. The point here is that there may be unacceptable costs (that is, solvable crimes going unsolved) associated with private police investigations.

While Benson concedes that private systems of law would suffer imperfections, he argues that in terms of "relative performance," there is simply no contest. It is unlikely that the *Enterprise of Law* will lead many readers to share Benson's conviction on this point because certain problems are not addressed adequately. But it is very likely that this book will leave many readers with the belief that the contest between public and private systems of law is much closer than they had originally thought.

Timothy Lynch
Cato Institute

Who Prospers?
How Cultural Values Shape Economic and Political Success
Lawrence J. Harrison
New York: Basic Books, 1992, 247 pp.

We live in an era of cultural egalitarianism; supposedly all cultures are equal, and cultural values do not matter. Ironically perhaps, our time is also marked by demands for special racial and ethnic rights, which threaten to balkanize America. Thus it is refreshing that Lawrence E. Harrison's new book risks political incorrectness and declares that culture does matter. Who can seriously deny that a society's prevailing attitudes toward self-responsibility, learning, work, entrepreneurship, achievement, the future, and trust of strangers heavily influence its economic success? The finest work on economic development—fron. such

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writers as Peter Bauer, Thomas Sowell, and Julian Simon—has long reflected the fact that cultural attitudes play a decisive role in determining the rate of material progress.

Harrison, who also wrote *Underdevelopment Is a State of Mind*, looks at the economic turnaround in Brazil, Spain, Taiwan, South Korea, and Japan in an effort to learn how culture encourages or impedes economic progress. For Brazil and Spain he finds that the progress-resistant Ibero-Catholic culture had to be overcome by contact with more progress-prone outside influences, especially through immigration. It was only when Spain and Brazil ceased to be isolated from the Protestant and Confucian work ethics that they prospered.

The discussions of Taiwan, South Korea, and Japan are more complex because of the cross-currents of Confucianism and the mitigating influence of Taoism. Confucius's ethical system combines esteem for learning, hard work, and one's progeny (the future) with a disdain for commerce and an admiration for authority. Taoism lauds frugality and individualism. Harrison theorizes how the progress-prone elements were accentuated and the progress-resistant ones diluted, leading to the phenomenal success of Taiwan and South Korea and of the Chinese everywhere but in China itself. He attributes those cultural metamorphoses to contact with the United States.

When Harrison discusses Japan, there are potential problems. He accepts the view that the Japanese psyche is founded on the Japanese approach to child rearing, which, "strongly rooted in Confucianism, emphasizes the child's membership in a group—initially the family—and tends to suppress individualism." Yet for Taiwan and South Korea, he credits Taoism with being "an individualistic protest against the hierarchical communitarianism and snobbery of Confucianism." That apparent contradiction could lead readers to conclude that for Harrison the same cultural value can be either progress-prone or progress-resistant depending, tautologically, on whether the people in question prospered or not. Even in the same country, he states, "the strong group emphasis and suppression of individualism characteristic of Japan have probably cut both ways with respect to innovation." (Curiously, according to Harrison, Japanese child rearing is "strongly permissive" as well as anti-individualist.)

Harrison also examines the economic performance of certain ethnic groups in the United States: the Chinese, Japanese, Koreans, Mexicans, and blacks. The first three groups have prospered, writes Harrison, because of their Confucian work ethic. Of course, it is also true that the people who uprooted themselves and moved to a strange culture were already determined to succeed. He reports that two-thirds of blacks have entered the middle class, and argues that the last third has lagged behind because of the lingering culture of slavery and Jim Crow. This prompts a question Harrison never takes up: Why didn't the one-third escape that culture the way the two-thirds did? Charles Murray provides an

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answer in *Losing Ground*—that the Great Society welfare programs created perverse incentives and stifled decades-long black economic progress—but Harrison gives this thesis barely a glance. In discussing how to elevate the black underclass, Harrison sensibly discards cultural relativism (going by the name of multiculturalism) as a counterfeit value. But his case for extending affirmative action for a limited time is sentimental and unpersuasive.

Harrison's real concern is America's Mexican population. Recent immigrants, legal and illegal, have come here to escape poverty, but have not rejected Iberian culture, he writes. Two and three generations later, they have not assimilated as fully as other groups have. Mexican-Americans, he alleges, are net consumers of government services. Thus, for Harrison, such immigration drags down the American economy. In fact, many studies show that generally immigrants, especially illegals, pay vastly more in federal, state, and local taxes than they get in government services. Harrison's only evidence is Michael Dukakis's profligate Massachusetts welfare state.

If Harrison is concerned about the Hispanics' failure to acculturate, he might have aimed his fire at the government programs that encourage their isolation by helping them to remain monolingual—in the name of bilingualism! (Orwell would have been amused.) Harrison's unsubstantiated case that immigration has helped cause America's economic "malaise" stems from his demonstrably false proposition that "population growth is a principal contributor to pressure on the environment." Julian Simon has refuted that claim. The problems associated with immigration are actually attributable to the welfare state. It would be better to dismantle the welfare state than to deprive foreign citizens of the chance to prosper and ourselves of the vitality of new immigrants. By advocating government measures to determine the size and character of the population, Harrison demonstrates the central planner's mentality and a failure to appreciate the dynamism and serendipity of the open-ended liberal order.

Harrison fears that America is losing the cultural values that made it prosperous. He blames much of this on the media's and academia's disdain for business. But paradoxically, he thinks the decline is principally the result of our affluence. (If he is right, he should favor immigration.) A more likely culprit is the ethos of the interventionist state and the loss of the vision of liberty that once united a culturally diverse America. (The "melting pot" is more legend that fact.) Today most people expect government to solve problems they never would have looked to government to solve a mere 30 years ago. Decades of propaganda about the benevolence and competence of the state have redefined the word "crisis" to mean that which only government can address. And today everything is in crisis.

Other problems in the book include Harrison's devotion to an "equitable" distribution of income (by which he seems to mean "equal") and

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his fondness for foreign aid. Harrison prefers a more equal income distribution to a less equal distribution, but he never says why or specifies the optimum distribution or explains how government can become involved in this matter without subverting the work ethic he elsewhere praises. He credits U.S. foreign aid with creating prosperity abroad, but seems oblivious of the substantial literature that questions the wisdom of giving bureaucrats taxpayer largess with which they can expand their public sectors.

Nevertheless, *Who Prospers?* affirms the importance of cultural values supportive of personal dedication to purpose, of entrepreneurship, and of economic progress. It is a worthwhile contribution.

Sheldon Richman
Cato Institute

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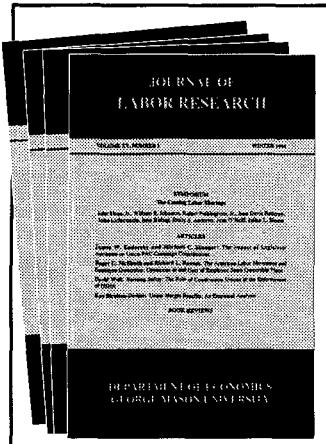

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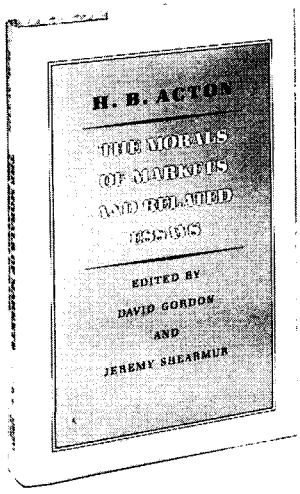

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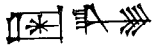

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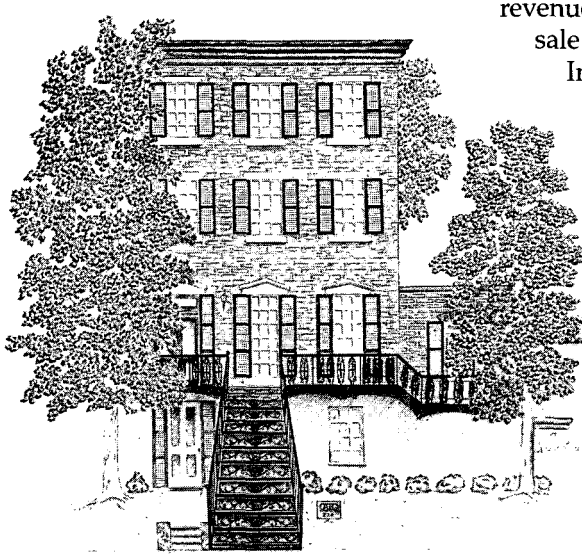

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THE INFORMATION PROBLEM

A predictable pattern assured by a well understood monetary order could . . . constrain monetary uncertainty and its real consequences. A monetary order thus emerges as an important institutional device to lower uncertainty and the associated information and inference problem confronting participants in the socio-economic game. Discretionary management ultimately fails to deliver, even with the best intentions, on its promise. The information problem separates the reality and the rationale of discretionary management by an unbridgeable gulf.

—Karl Brunner

"Monetary Policy and Monetary Order," in *Monetary Policy and Monetary Regimes*, (Rochester, N.Y.: Graduate School of Management, University of Rochester, 1985), p. 12.

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